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A

# COMPLETE TREATISE

ON

# ELECTRICITY,

IN

THEORY AND PRACTICE;

WITH

ORIGINAL EXPERIMENTS.

By TIBERIUS CAVALLO, F.R.S.

IN THREE VOLUMES.

VOLUME III.

*Containing the* DISCOVERIES *and* IMPROVEMENTS MADE SINCE THE THIRD EDITION.

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THE

# PREFACE.

THE rapid advances made in the Science of ELECTRICITY ſince the firſt publication of this Treatiſe, have furniſhed conſiderable additions for every ſubſequent edition. Thoſe additions having been interſperſed throughout the Work in the three firſt editions, have obliged ſeveral perſons, who were deſirous of being informed of the new improvements, to purchaſe the Work more than once. In order to avoid this inconvenience,

 the

the preſent edition has been publiſh-ed in *three volumes*; the firſt and ſe-cond of which have been reprinted without any material alteration, whilſt the new materials are contained in this *additional* volume, which, inde-pendent of its being ſold with the other two, may be purchaſed by itſelf, to complete the third edition.

Beſides a great many other articles, the Reader will find in this volume an account of the new ſubject of ANIMAL ELECTRICITY, which may be juſtly conſidered as one of the greateſt diſcoveries made in the preſent century.

THE

# CONTENTS

OF

## VOLUME III.

## NOTES AND ADDITIONS TO VARIOUS PARTS OF THIS TREATISE.



IX. *Pre-*

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*N. B.* When the whole of this volume was printed, I had an opportunity of perusing Mr. SAUSSURE's Experiments and Observations on the Atmospherical Electricity, and on the Electricity which is produced by the evaporation of water, &c. I shall therefore just mention, that this ingenious philosopher, having made a variety of well-conducted experiments on evaporation, found, that positive Electricity was produced by the evaporation of water from some other heated substances, besides those mentioned in N° V. page 270 of this Volume. Positive Electricity was produced when he dropped a lump of red-hot iron into a small quantity of water; but when water was thrown at successive intervals into a large iron crucible highly heated, then no Electricity appeared at first; some time after a positive Electricity was manifested, and at last the Electricity became negative. Copper did likewise produce the two Electricities; but silver, and porcelain produced the negative Electricity.

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A COMPLETE

## AN ACCOUNT

OF THE

## *Discoveries concerning Muscular Motion*

WHICH HAVE BEEN LATELY MADE,

AND ARE COMMONLY KNOWN UNDER THE NAME OF

# ANIMAL ELECTRICITY.

THE subject of muscular motion, which has exercised the genius of philosophers from time immemorial, is one of those arcana of nature, that have hitherto eluded the investigation of human industry. Innumerable experiments have been made in vain, and the labours of one man have hardly ever produced any thing more than the refutation of another man's hypothesis.

This labyrinth of obscurity has at last received a ray of light from the recent discoveries of Dr. Galvani of Bologna. A new way has been opened to promising

experimental inveſtigation, and many ingenious perſons throughout Europe are now purſuing the track with care and aſſiduity.

The original diſcoveries were announced to the world in a quarto book, conſiſting of fifty-eight pages with four large plates, and entitled, *Aloyſii Galvani de Viribus Electricitatis in motu muſculari Commentarius. Bononiae* 1791. Abſtracts of this work were ſoon after inſerted in various periodical publications, and theſe have been followed by farther diſcoveries made by other ingenious perſons, and eſpecially by Profeſſor Volta, Dr. Euſebius Valli, Dr. Monro, and Dr. Fowler. The ſcattered materials are numerous for want of a theory; but neither can a theory be formed, nor even farther inveſtigation be inſtituted, without a comprehenſive view of all that has been done concerning the preſent experiments.

Having myſelf made ſeveral experiments on this intereſting ſubject, moſtly in conjunction with Dr. James Lind, F. R. S. I have in a manner been under the neceſſity

ceſſity of collecting and methodiſing not only the facts that have appeared in print, but thoſe alſo which have been communicated to me through letters, or by means of verbal information; I think it neceſſary to add this account to my *Treatiſe on Electricity,* and hope that a compendious view of the ſubject may be of uſe to thoſe perſons, who are diſpoſed to engage in ſo promiſing a field of curious and uſeful inveſtigation.

It is for the ſake of brevity, and as this account is not intended to be hiſtorical, that I ſhall not follow the order of time, nor particularly point out the perſon to whom each diſcovery or obſervation belongs. Their names will undoubtedly be recorded, with praiſe and gratitude, in many ſubſequent publications.

Previous to theſe diſcoveries, the name of *Animal Electricity* was given to the analogous powers of three fiſhes; namely, the *Torpedo*, the *Gymnotus Electricus*, and the *Silurus Electricus*.—Whenever a communication is made by means of ſubſtances that

are Conductors of electricity, between one part and another of those fishes, and a man or other animal is placed in the circuit, which forms the said communication; the man or other animal feels a shock analogous to that occasioned by the discharge of a Leyden phial. If the circuit of communication be interrupted, or formed of substances which are not Conductors of electricity, the shock is not felt, or at least a very slight tremor is perceived in the hand that touches the animal. The gymnotus alone affords a luminous spark, which becomes evident by making an exceedingly small interruption in the circuit of communication. Excepting these properties, it is not known that they have any thing in common with artificial electricity; and every property vanishes with the entire death of the animal *.

The

* A fourth electrical fish was found on the coast of Johanna, one of the Comoro islands, in lat. 12° 13′ South, by Lieutenant WM. PATERSON, and an imperfect account of it was published in the 76th Vol. of the Phil. Transf.

" The fish *is described* to be seven inches long, 2½
" inches

The diſcoveries of Dr. Galvani were made principally with frogs, and after the animals were dead. He firſt diſcovered, that a frog dead and ſkinned, is capable of having its muſcles brought into action by means of artificial or atmoſpherical electricity. And, ſecondly, he diſcovered, that independent of any artificial or natural Electricity, the ſame motions may be produced in the dead animal, or even in a detached limb, by merely making a communication between the nerves and the muſcles, with ſubſtances that are Conductors of electricity. If the circuit of

"inches broad, has a long projecting mouth, and ſeems "of the genus Tetrodon. The back of the fiſh is "a dark brown colour, the belly part of ſea-green, "the ſides yellow, and the fins and tail of a ſandy "green. The body is interſperſed with red, green, and "white ſpots, the white ones particularly bright; the "eyes large, the iris red, its outer edge tinged with "yellow."

Whilſt this fiſh is living, ſtrong ſhocks, like electrical ſhocks, are felt by a perſon who attempts to hold it between his hands. Three perſons are mentioned in the account to have experienced this property of one of thoſe fiſhes; but the want of opportunity prevented the trial of farther experiments.

communication be made by means of Non-conductors of electricity, as glaſs, ſealing wax, and the like; no motion will take place.—It is owing to theſe circumſtances, that the name of Animal Electricity has been beſtowed on this wonderful property of organized animal matter.—Theſe properties have been verified in a great many other animals beſides frogs.

Previous to Galvani's diſcoveries, I find only one curious fact recorded, which ſeems to be materially connected with the preſent ſubject. It is related in a letter of Dr. Cotugno, Profeſſor of Anatomy at Naples, to the Chevalier Vivenzio of the ſame place. This letter, a tranſlation of which I ſhall ſubjoin, was publiſhed in the Italian tranſlation of a book of mine on Electricity, by the above-mentioned Chevalier Vivenzio.

"SIR,

"THE obſervation, which I mentioned ſome days ago, when we were diſcourſing together of the electrical animals, upon which I ſaid that I believed the mouſe to be one of the number, is the following:"

"Towards

"Towards the latter end of March I was ſitting with a table before me; and obſerving ſomething to move about my foot, which drew my attention, looking towards the floor, I ſaw a ſmall domeſtic mouſe, which, as its coat indicated, muſt have been very young. As the little animal could not move very quick, I eaſily laid hold of it by the ſkin of the back, and turned it upſide down; then with a ſmall knife that laid by me, I intended to diſſect it. When I firſt made the inciſion into the epigaſtric region, the mouſe was ſituated between the thumb and firſt finger of my left hand, and its tail was got between the two laſt fingers. I had hardly cut through part of the ſkin of that region, when the mouſe vibrated its tail between the fingers, and was ſo violently agitated againſt the third finger, that to my great aſtoniſhment I felt a ſhock through my left arm as far as the neck, attended with an internal tremor, a painful ſenſation in the muſcles of the arm, and with ſuch giddineſs of the head, that being affrighted, I dropped the mouſe. The ſtupor of the arm laſted upwards of a quarter of an hour,

nor could I afterwards think of the accident without emotion. I had no idea that ſuch an animal was electrical; but in this I had the poſitive proof of experience."

*Naples, Oct. 2d.* 1784.

1. The action of Electricity on a dead and ſkinned frog (and indeed on other animals more or leſs) occaſions a tremulous motion of the muſcles, and generally an extenſion of the limbs.

2. When the nerves of a frog recently killed and deprived of its integuments, are expoſed to an electrified atmoſphere, or, in ſhort, are ſo diſpoſed, as that by the action of an electrical machine, or of any electrified body, a quantity of electric fluid is cauſed to paſs through them, a contraction of the muſcles takes place, with a tremulous convulſive motion, which may be reiterated for ſome hours after. Dr. Galvani prepared a frog, having its legs attached to a part of the ſpine, but ſeparated from all the reſt of the body; and obſerved that whenever a ſpark was taken from a large prime Conductor of an electrical machine ſituated at ſome

ſome diſtance from the prepared part of the animal, thoſe legs moved with a kind of ſpaſmodic contraction, ſometimes ſtrong enough to jump a conſiderable way. It was found neceſſary to place the prepared legs contiguous to ſome good Conductor not inſulated.

3. Whether the frog be brought into actual contact with the electrified body, or not; whether it be made to receive the ſpark itſelf or not, the motions happen equally well, provided a quantity of electric fluid be cauſed to paſs through it, which may be done merely by the preſſure or action of electric atmoſpheres.—When the electricity is made to paſs through the frog, by the immediate contact of the electrified body, a much ſmaller quantity of it is ſufficient to occaſion the movements, than when it is made to paſs from one Conductor to another at a certain diſtance from the prepared animal.

4. If the electric atmoſphere be ſo ſtrong as to occaſion little ſparks between the Conductors contiguous to the animal, or if it

be

be capable of affecting an electrometer placed near the animal; then even a whole frog, a lizard, a mouſe, or a ſparrow, will be ſtrongly affected with violent convulſions. When the animal is inſulated, and the electricity is made to paſs through its body, then a whole living frog is affected by the paſſage of ſo ſmall a quantity of electricity as is diſcharged by a middling prime Conductor, that is juſt capable of affording a ſmall ſpark. In this caſe, if a Leyden phial be uſed, a much ſmaller quantity of electricity will be found ſufficient for the purpoſe, viz. ſuch a charge of it as cannot afford a ſpark, but that can juſt produce a ſenſible divergence of the pendulums of an electrometer.

5. But a frog prepared, eſpecially after the manner of Dr. Galvani, is affected by an incomparably ſmaller quantity of electricity. Mr. Volta has obſerved, that ſo ſmall a quantity of electricity as is abſolutely incapable of occaſioning a divergence in the moſt ſenſible electrometer, but ſuch as may be obſerved by his condenſer of electricity, is

is ſufficient for the purpoſe *. Thus if a Leyden phial be charged, and diſcharged, and afterwards be diſpoſed, ſo that the prepared frog be placed in the circuit between its inſide and outſide coating; the paſſage of that ſmall reſiduum is fully ſufficient to produce the contractions, &c.— By being ſenſible of ſo ſmall a quantity of electricity, the prepared frog becomes a moſt ſenſible ſort of electrometer, which perhaps hereafter may be of ſingular uſe in ſome nice electrical experiments.

6. When the preparation and diſpoſition of the frog is ſuch, as that the electric fluid

* Mr. Volta eſtimates the quantity of electricity, which can be perceived by means of his condenſer, to be the 500dth or 600dth part of that quantity, which is neceſſary for affecting my electrometer. Upon this I muſt obſerve, not for the ſake of comparing the condenſer with the electrometer, but merely to convey a juſt idea of the quantity of electricity neceſſary for the preſent purpoſe; that Mr. Volta's condenſer cannot render ſenſible a quantity of electricity abſolutely ſmall, but collects together, and condenſes a certain quantity of it, which, by being diffuſed through a conſiderable ſpace, exiſts in ſo rare, or uncondenſed a ſtate, as not to be capable of affecting an electrometer.

muſt paſs through a nerve to the muſcle or muſcles, then the movements are in general much ſtronger, than when it is applied to any other part of the body.

7. Dr. Galvani had the curioſity of trying whether the electricity of the clouds produced the ſame effect on the prepared limbs, as the artificial electricity of the ordinary machines, and for that purpoſe he extended a Conductor from the top of a houſe to the prepared animal, which was ſometimes laid on a table in the open air, and at other times was encloſed in a glaſs receiver. On this preparation the thunder and lightning produced the ſame effects as the ſparks from the electrical machine. The ſame contractions took place, and they were ſtronger or weaker according to the diſtance and quantity of lightning. Thus far the effects might have been naturally expected; but a remarkable circumſtance was obſerved, which ſerves to explain another phenomenon of nature. — It was found, that inſtead of one contraction at every clap of thunder, the limbs were affected with a ſort of tremor or ſucceſſion of convulſions,

vulſions, which ſeemed to be nearly equal in number to the repetition of the thunder, viz. that ſucceſſion of exploſions which forms the rumbling noiſe of thunder *. Now this obſervation proves, that the rumbling noiſe is not the echo of a ſingle exploſion, or the ſucceſſive arrival of the vibrations produced at different diſtances, though at the ſame moment of time; but that it is produced by a quick ſucceſſion of ſeveral exploſions, which indeed ſeems to be confirmed by obſerving, that the clouds are very imperfect Conductors, in which ſtate they are not likely to receive a full and ſingle ſtroke of electricity from other clouds, or from the earth.

8. The ſenſibility of the prepared animal is greateſt at firſt, but it diminiſhes by degrees till it vaniſhes intirely. In general, frogs, and other animals with cold blood, retain the property of being affected by electricity much longer than thoſe poſſeſſed of hot blood. With ſome of the latter, the ſenſibility is very weak, and will hardly

* Galvani de viribus electricitatis in motu muſculari, p. 15.

laſt for a few minutes after the death of the animal; whereas ſome of the animals with cold blood, and eſpecially frogs, which are by far the fitteſt animals for ſuch experiments, have frequently retained that property for upwards of 12 hours, and ſometimes even for two or three days.

9. Thus far we have deſcribed the effects of electricity on dead animals; we ſhall now come to the moſt curious part of the ſubject, which is, that the ſame motions, the ſame convulſions, &c. and for a time about equally long, can be produced in dead, and even in living animals, without the aid of any apparent electricity.—In an animal recently dead, detach a nerve from the ſurrounding parts; taking care to cut it not too near its inſertion into the muſcles; remove the integuments from over the muſcles depending on that nerve; take a piece of metal, as a wire, touch the nerve with one extremity, and the muſcles with the other extremity of the wire, and the conſequence will be, that the muſcles will move exactly as if a quantity of electricity were ſent through them. This experiment will

will anſwer equally well when the preparation is laid upon an inſulated ſtand, as when it communicates with the ground. If the communication between the nerve and the muſcles, inſtead of being formed by means of metal or other Conductor of electricity, be formed of ſubſtances that are non-conductors, as glaſs, ſealing wax, oils, &c. then no motion will take place.

10. When the application of the metal or metals is continued ſteadily on the part, the contractions will ceaſe after a certain time, and on removing the metal, ſeldom, if ever, any contraction is obſerved.

11. Our ignorance of any other power in nature excepting electricity, that can be tranſmitted with great rapidity through metals, water, &c. and that cannot be tranſmitted through glaſs, reſins, and certain other ſubſtances, induces one to imagine, that the above-mentioned effects are owing to electricity, generated in ſome part or other of the animal or of the ſurrounding bodies, as the metals or other bodies which form the communication between the nerves

and

and mufcles *. It is not eafy to conceive how a quantity of electric fluid can be generated or accumulated in one part, and a deficiency of it can take place in another part, of the body of an animal, which is in every part a Conductor of electricity; and even if it confifted of Conductors, and of Non-conductors combined together, it would be difficult to account for that quick production of electricity, which the effects obferved in thefe experiments feem to require.

12. However, notwithftanding the obfcurity in which the fubject is at prefent involved, I muft take the liberty of mentioning a confideration which occurred to me when I firft heard of thefe difcoveries, and which may probably be of ufe to others in the inveftigation of fo wonderful a phenomenon.—Electricians have divided the

* Heat is more eafily conducted by certain bodies than by others, and thofe bodies which tranfmit it more eafily are in general Conductors of electricity alfo; but the flownefs with which heat is propagated, befides other obvious confiderations, feem to exclude it from the fuppofition of its being effentially concerned in the prefent experiments.

various

various ſubſtances of our globe into Conductors and Non-conductors of electricity. The great difference between their peculiar properties is certainly evident and uſeful; ſince, without ſuch a difference, we could not have had any knowledge of the exiſtence of electricity. But, ſtrictly ſpeaking, there is no ſubſtance in nature, that may be called a perfect Conductor, or a perfect Non-conductor of electricity. The various ſtates of the ſame ſubſtance, with reſpect to heat and cold, or of rarity and conſiſtency, are attended with a conſiderable variation of its conducting or non-conducting property. Glaſs itſelf becomes a Conductor by being heated to a certain degree, and any one of the metals oppoſes a certain reſiſtance to the paſſage of the electric fluid *. But omitting thoſe bodies which are the moſt perfect of their claſs, let us conſider a caſe in which ſome electricity is produced and diſſipated amongſt ſubſtances that are leſs perfect.—When a perſon with a dry hand rubs a piece of paper ſufficiently

* For proofs of this aſſertion, ſee the firſt volume of this Treatiſe.

dry, ſome electricity will be produced or accumulated upon the paper. If the paper thus electrified be held by one corner, the electricity will preſently diſappear. This effect is eaſily explained, by conſidering the imperfect conducting and non-conducting ſtate of the bodies concerned. The friction of the hand accumulates ſome electricity upon the paper; but the paper being an imperfect Conductor, cannot carry away to the other hand that holds it, all the electricity as quickly as it is accumulated upon it; the friction being interrupted, the electricity is gradually diſſipated. When the paper is leſs dry, the electricity will remain for a ſhorter time, or it may be conducted away as quickly as it is generated. Now, in order to apply this effect to the caſe of animal electricity, we muſt firſt conſider, that though the whole animal is a Conductor of electricity, yet every part of it is not an equally good Conductor. Admitting then, or ſuppoſing, that in the body of the animal a quantity of electricity is produced by ſome cauſe or other to us unknown, it muſt follow, that this electricity muſt be conducted by, or expanded through, ſome parts

parts much eaſier than through others; conſequently the former will contain more of that power, than the latter. The metal, then, which is applied to form the communication between the former and the latter, being a better Conductor than either, reſtores the equilibrium, and thus the animal electricity itſelf may produce the effects of the artificial electricity.—This conſideration removes one difficulty, viz. it ſhews that an unbalanced ſtate of electricity may take place, and that the equilibrium may be reſtored in a body, or congeries of ſubſtances, which are all Conductors, though not equally good Conductors of electricity.

13. The conducting communication between the muſcle and the nerve may conſiſt of one or more pieces, and of the ſame or of different bodies connected together, as metals, water, a number of perſons, and even wood, the floor, and walls of a room. But it muſt be obſerved, that the leſs perfect Conductors will anſwer only at firſt, when the prepared animal is vigorous; but when the power begins to diminiſh, then only the more perfect Conductors, as the

metals, will anſwer, and even theſe are attended with various effects.

14. It is in this nearly exhauſted ſtate of the animal electricity, that the various conducting powers of different ſubſtances can be obſerved; and thus amongſt the experiments which I made with Dr. LIND, we formed the following liſt of Conductors, which are arranged in the order of their perfection, beginning with the beſt. Yet I do not mean to offer this as a very correct arrangement; for though it has been deduced from a great number of experiments, their reſult has not however been very conſtant. A conſiderable difference is frequently occaſioned by circumſtances that are hardly perceivable; ſuch as the changeable ſtate of the prepared animal, the ſurface of the ſubſtances uſed, the quantity of contact, &c.

Malleable platina.
Silver.
Gold.
Quickſilver.
Copper.

Braſs.

Brafs.
Tin.
Lead.
Iron.
The human body.
Salt water.
Frefh water.

15. The metallic ores are not fo good Conductors as the purified metals themfelves, and their conducting power is various according to the nature of the ores, but even the metallic falts are tolerably good Conductors.

16. It is very remarkable, that the flame of a tallow-candle, which is a good Conductor of common electricity, will not conduct the animal electricity, when placed in a fhort interruption made in the circuit of communication. Charcoal placed in the fame fituation as the flame of the candle, was alfo found to be a Non-conductor, except when it was actually burning, in which ftate it conducted tolerably well; but Mr. VOLTA fays, that he has found fome pieces of charcoal that acted as well as the metals.

—Dr. VALLI obſerved, that human bodies are not all equally good Conductors. Out of four perſons in a company, he found that when two of them formed the circuit of communication between the nerve and the muſcles of a frog, the motions took place very well. When a third perſon formed the circuit, the motions were very weak; but that when the fourth perſon formed the communication, no motion took place. This experiment, he adds, was often repeated with the ſame ſucceſs. It may however be obſerved, that the greater or leſs dryneſs of the ſkin of the hands in thoſe perſons, might have been ſufficient to produce the obſerved diverſity of effect.

17. Vitriolic acid, and, what is very remarkable, alcohol, appear to conduct this property rather better than water.

18. This power, like the common electricity, paſſes through the ſubſtance, and not only over the ſurface of Conductors; hence a wire ſurrounded with ſealing-wax, or other non-conducting ſubſtance, except where it touches the animal preparation, will

will anſwer as well as when it is not coated with a non-conducting ſubſtance.

19. When various Conductors are placed contiguous to each other in the circuit between the muſcle and the nerve, their contact muſt be perfect; otherwiſe the deſired effect will not take place. To lay one metal upon the other is ſeldom ſufficient, unleſs they are preſſed againſt each other. If two or more perſons join hands, the contact muſt be frequently rendered more perfect by the interpoſition of water, viz. by moiſtening the fingers, eſpecially with ſalt water. Sometimes when the fingers are greaſy, or have much perſpired, the animal electricity will not be communicated through them. In this caſe, it becomes neceſſary to waſh them well, and then to dip them in ſalt water. An interruption, that could not exceed the 200th part of an inch, which I made in the metallic circuit of communication, did abſolutely prevent the communication of the power between the nerves and muſcles of ſix frogs, which had been prepared and connected all together.

 20. The

20. The arteries and the veins are not ſo good Conductors as the nerves; for when a blood veſſel forms part of the circuit of communication, the contractions will take place only when nervous ramifications are adhering to it, and if theſe be carefully ſeparated, the motions will not happen.—The ſame thing may be ſaid of the tendons, the bones, and the membranes; for when either of thoſe parts is ſeparated from the body, and is introduced in the circle of communication between the muſcles and nerves of a prepared frog, no motion will enſue, excepting indeed when thoſe parts are full of moiſture, and are in immediate contact with the nerve of the prepared frog.—Dry nerves are not Conductors of animal electricity. Dr. Valli found, that the internal ſubſtance of a nerve conducts much better than its external, or coat.

21. If part of the nerve be wrapped up in a thin piece of metal, as tin-foil or ſheet-lead, and the conducting communication, or conducting metallic rod, be applied from this *coating* or *armour* to the muſcles, the motions will, in that caſe, be much ſtronger.

The

The muſcle itſelf may alſo be armed with, or ſimply laid upon, metal, and on compleating the communication between the armour of the nerve and that of the muſcle, or muſcles, the motions will be very vigorous, and will continue much longer, than when no coating or armour is uſed. Beſides metals, the armour may conſiſt of water or of other Conductors, as will be ſhewn in the ſequel. The uſe of this armour ſeems to be the augmentation of the points of contact. The effect has been obſerved to be much greater, and to ſucceed more conſtantly, when the conducting rod is put firſt in contact with the muſcle or its coating, and is then brought with its other extremity into contact with the armour of the nerve, than when it is placed in contact with the nerve firſt. Hence, when the power of the animal is much weakened, the former way will anſwer, but the latter will not.

22. It is very ſingular that in this experiment it is neceſſary to employ two different metals, viz. One to be in contact with the nerve, and another to be in con-

tact

tact with the muſcles; for if they be of the ſame ſort, as both of ſilver, or both of tin-foil, the contractions will not take place. It muſt however be obſerved, that in the beginning, when the power of the prepared animal is ſtrong, the convulſions will happen even when the two coatings are of the ſame ſort of metal; though not nearly ſo well as when two metals are uſed. But indeed in the beginning, when the animal electricity is ſtrong, the motions are frequently produced without any coatings, and even without any conducting rod. The mere ſtriking of the table, or the approach of a piece of metal, without any actual contact, will frequently excite the movements*. But this ſtrong ſenſibility is of ſhort duration; after which period the two armours of the ſame ſort of metal will not occaſion any motion whatever.—The leaſt difference, however, in the quality of the two coatings, is ſufficient to produce weak motions, as when they are of ſilver of different degrees

* "I have likewiſe, *ſays Dr.* VALLI, ſeen in two frogs "the movements occur at the diſtance of half an inch "from the ſciſſars, and which ceaſed at the moment I "inſulated the ſciſſars." Exp. on An. El. p. 40.

of

of purity, or of different ſorts of lead, &c. —It is for the ſame reaſon, that if they conſiſt of two metals that have a great affinity to each other, the effect is not ſo great as when the two metals are more diſſimilar in their nature. Thus it has been found, that gold and ſilver do not anſwer ſo well as ſilver and zinc, or gold and lead. Either gold or ſilver, or ſteel, or copper, or molybdena, when combined with tin or lead, or eſpecially with zinc, are very good exciters of the contractions in prepared animals. But the combinations of any two of the former metals are much inferior in power*. —Large pieces of the metals, and with ample ſurfaces, ſeem to anſwer better than ſmall and compact pieces for theſe experiments; for with the former contractions may be excited, when the latter are unable to produce any effect. Farther experiments

* Dr. VALLI ſays, that with the prepared fore-legs of a rabbit, he found that the following combinations of metals produced no movements, viz. glaziers lead, and aſſay lead, lead and iron, lead and gold, lead and copper, lead and zinc, or lead and antimony. Experiments on An. El., p. 45. But it ſeems that thoſe concluſions were not grounded upon a ſufficient number of experiments.

are however required to aſcertain all the particulars relative to this part of the ſubject.

23. The abovementioned ſingular properties, together with ſome other facts, which will be mentioned in the ſequel, induced Mr. VOLTA to ſuſpect, that poſſibly in many caſes the motions are occaſioned by a ſmall quantity of electricity produced by the mere contact of two different metals; though he acknowledges, that he by no means comprehends in what manner this can happen. This ſuſpicion being entertained by ſo eminent a philoſopher as Mr. VOLTA, induced Dr. LIND and myſelf to attempt ſome experiments, which might verify it; and with this view we connected together a variety of metallic ſubſtances in diverſe quantities, and that by means of inſulated, or not inſulated communications; we uſed Mr. VOLTA's condenſer, and likewiſe a condenſer of a new ſort; the electrometer employed was of the moſt ſenſible ſort; and various other contrivances were uſed, which it will be needleſs to deſcribe in this place; but we could never

* obtain

obtain the ſmalleſt appearance of electricity from thoſe metallic combinations. Yet we can infer no other concluſion, but that if the mere combination, or contact, of two metals produces any electricity, the quantity of it in our experiments was too ſmall to be manifeſted by our inſtruments.

24. It has been lately aſſerted, that by means of the *doubler of electricity* (an inſtrument of which particular notice will be taken in the courſe of this volume) the production of different ſorts of electricity, by the mere contact of different metals, has been fully aſcertained; and that zinc has been found to communicate poſitive electricity, whilſt ſilver gives the negative. If this diſcovery has really been made, we muſt acknowledge it to be a very conſiderable improvement in the ſcience of electricity; but, for my part, I muſt confeſs that my endeavours to prove it, either by a doubler of electricity made in the moſt unexceptionable manner, or by other inſtruments, have as yet proved unſucceſsful; nor have I met with any perſon who could

ſhew

ſhew me ſo ſingular a property of metallic ſubſtances.

25. The motions will be alſo excited when the metals are not in immediate contact with the prepared limb, provided they form part of the circuit of communication. Thus lay a prepared limb upon a table, hold the nerve in one hand, and a piece of zinc in the other hand: lay a piece of ſilver upon the table at about one or two inches diſtance from the prepared muſcles, and make a communication between the muſcles and the ſilver by means of water or ſome other good Conductor. If now you touch the ſaid piece of ſilver with the zinc which you hold in one hand, the contractions will take place. The ſame effect will happen, if the two pieces of metal be firſt put in contact, and then the operator touches the nerve of the preparation with his finger.

26. The preparation of the frog, or other animal, for this experiment, generally conſiſts in detaching one of the principal nerves from all the ſurrounding parts, where it enters

ters a member ſuſceptible of motion, and arming it with a metallic foil. On making the communication, the motion will take place; but the preparation which anſwers beſt, is delineated in *fig.* 1 *and* 2 *of the plate*, which, for the ſake of diſtinction, we ſhall in the following pages, call *the uſual preparation*; it being, in fact, that which has been more frequently and more advantageouſly uſed.—It is made in the following manner.—Separate with a pair of ſciſſars the head and upper extremities of a frog, in the line AB, from the reſt of the body. Open the integuments and muſcles of the abdomen, and remove the entrails; then you will lay bare the crural nerves, as ſhewn in fig. 1*ſt*. which in this animal come out of the ſpine at a conſiderable diſtance above the pelvis; viz. from the line CD. Then paſs one blade of the ſciſſars under the ſaid nerves, and cut off the ſpine with the fleſh cloſe to the thighs in the line EF, by which means the legs will remain attached to the ſpine by the nerves alone. This done, leave only a ſmall bit of the ſpine attached to the crural nerves, and cut off all the reſt. Thus you will have the two legs GH, *fg* 2.

of a frog adhering to a bit of the ſpine ACD, by means of the crural nerves CE, DF. Theſe legs muſt be flayed in order to lay bare the muſcles. The metallic armour, which generally conſiſts of a piece of tin-foil, muſt be placed round the nerves very near the ſpine, viz. at CD, or round the whole bit of ſpine AD, and the extremities of the nerves next to it. A frog thus prepared, and touched by means of a conducting rod applied to the muſcles and to the armour of the nerves, will act vigorouſly for a conſiderable time. Some contractions have been obſerved ſeveral hours, and even days after; but the power is gradually diminiſhing, and in general it can ſeldom be perceived after two or three hours.

27. With a frog prepared in the above deſcribed manner, one may ſhew the experiment various ways; but the two following methods are peculiarly eligible, becauſe they produce very ſtrong and ſtriking movements.—Hold the preparation by the extremity of one leg, the other leg hanging down, with the armed bundle of nerves

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and bit of ſpine lying upon it. In this ſituation interpoſe a piece of ſilver, as a half-crown, between the lower thigh and the nerves, ſo that it may touch the former with one ſurface, and the metallic coating of the latter with the other ſurface, or with its edge; and you will find that the hanging leg will vibrate very powerfully, ſometimes ſo far as to ſtrike againſt the hand, which holds the other leg.

28. The other method is the following: Put two wine-glaſſes full of water contiguous to each other, but not actually touching. Place the thighs and legs of the prepared frog in the water of one glaſs; and laying the nerves over the edges of the two glaſſes, let the bit of ſpine and armour touch the water of the other glaſs. This done, if you form the communication between the water of the two glaſſes by means of the conducting rod, or put the fingers of one hand into the water of the glaſs that contains the legs; and holding a piece of ſilver in the other hand, touch the coating of the nerves with it, you will

 find

find that the prepared legs will move ſometimes ſo powerfully as to jump fairly out of the glaſs. It is however neceſſary to remark that the effect in this experiment is increaſed by a ſort of artifice, which muſt neceſſarily take place; viz. the legs, in order to be placed into the glaſs, muſt neceſſarily be drawn up, and as the effect of the communication, &c. is to produce a ſudden extenſion of the limbs, it muſt naturally follow that from the reſiſtance of the glaſs, the preparation muſt endeavour to jump out of the water.—There is another reaſon, which contributes to increaſe the effect in this experiment, which is, that the water, by ſurrounding the prepared legs, becomes a very ample armour or coating, which touches the legs in every point of their ſurface.—It frequently happens in this experiment, when the Animal Electricity is ſtrong, that without forming the communication, the motions are excited by only touching the armour of the nerves with a piece of metal. Or if one perſon touch the coated ſpine, whilſt another perſon touches the legs, the motions will take

place, though the two perſons do not communicate with each other by any thing elſe beſides the floor.

29. In theſe experiments the effects are exactly the ſame, whether they be performed on a table in the ordinary way, or in an electrified atmoſphere, viz. when the preparation, operator, &c. are all placed on an inſulating ſtool, and are kept electrified by the action of an electrical machine.

30. We have ſaid above, that whenever a limb of an animal is prepared, and the communication is formed between the nerve and the depending muſcle, the contractions will take place; but we muſt now take notice of a very remarkable exception; which is that thoſe parts, the motion of which is ſubject to the will of the animal, are ſuſceptible of contraction by the application of metals; but of the involuntary muſcles, the heart alone is capable of being contracted. This peculiar property of the heart was ſatisfactorily proved by Dr. FOWLER, with the hearts of frogs, cats, and rabbits. "At length," *ſays he*,

 "I was

"I was ſo happy as to ſucceed completely. "On the 18th of March laſt, in preſence "of my friends, Mr. Hunter, and Mr. "Thomson, having diſſected away the pe-"ricardium from a frog's heart, which had "an hour before ceaſed ſpontaneouſly to "contract, I removed the muſcles, and "cellular membrane covering its nerves, "and large blood-veſſels; I then placed one "end of a rod of pure ſilver in contact "with one ſide of theſe nerves and blood-"veſſels, and one end of a rod of zinc "on the other, both of them at about the "diſtance of the third part of an inch "from the auricles of the heart. On "bringing the oppoſite ends of theſe rods "in contact with each other, the auricle "firſt, and then the ventricle of the heart, "immediately contracted, and repeated their "contractions as often as the ends of the "metal rods were made to touch each "other."——"The contractions were "both more vigorous, and more conſtant, "when the metals were placed in contact "with the heart itſelf, than when touching "only its blood-veſſels and nerves."

"In

"In order to the complete ſucceſs of this "experiment, it is neceſſary that the ſpon- "taneous contractions of the heart ſhould "nearly, if not altogether, have ceaſed; "and when in this ſtate, the experiment "is rendered ſtill more ſatisfactory by re- "moving the heart from the body of the "frog, and laying it upon a plate of "zinc*."

31. It has been obſerved, by way of aſſiſting the inveſtigation of the above remarked ſingularity, that the muſcles which are not ſubject to the will, do not poſſeſs ſo large and ſo many nerves as the other muſcles.

32. The application of the metallic rod to the prepared nerve and depending limb, does not produce contractions in that limb only; but it contracts ſeveral other parts that are left attached to it. Thus, if the crural nerve be detached and armed with metal, whilſt every other part of the animal is left untouched, on applying the me-

* Fowler on An. Elec. p. 75, 76, and 77.

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tallic rod to the ſaid nerve, and to the muſcles of the leg, the upper as well as lower limbs will contract: even the eye-lids, and other parts of the head, will be ſeen to move *.

33. Dr. MONRO obſerves, that the application of metals to the head of a frog, or to any part of its ſpine above the ſixth vertebra, does not occaſion any convulſions of its hind legs; by which he is led to ſuppoſe that the nerves of the hind legs are not derived ſolely or chiefly from the brain or *cerebellum*.

34. By repeatedly applying the conducting rod to a prepared animal, its power is exhauſted much ſooner, than if it be uſed more ſparingly. It is very remarkable, that when a prepared frog is almoſt exhauſted of its Animal Electricity by the often repeated application of the metals, its power will be in a great meaſure reſtored by leaving it at reſt for ſome time. This is analogous to the recovery of ſtrength,

* GALVANI de viribus El. in m. m. p. 28.

which

which reſt alone can produce in living animals, when over-fatigued, and may probably depend upon the ſame cauſe.

35. When a frog, prepared in the uſual manner, is almoſt exhauſted of its power by the often repeated application of the conducting rod, remove the armour to another part of the ſame nerve, eſpecially if it be nearer to the muſcles, and you will find the power in a great meaſure reſtored. This is a curious obſervation, and naturally leads us to inquire, what does the nerve loſe by the application of the armour in this experiment?—It looks as if that part of the nerve were alone concerned in the production of the power called Animal Electricity.

36. A ligature made on the nerve cloſe to its inſertion into the muſcle, frequently prevents the motions; but if it be made at a diſtance from the muſcle, the experiment ſucceeds as well as without the ligature.

37. Dr. VALLI ſays that the ligature of the nerve oppoſes the ſame obſtacle to Ar-

 tificial,

tificial, as it does to Animal Electricity; for when the ligature was in contact with the muſcles a very great quantity of electricity was found neceſſary to produce the movements, in compariſon with that quantity of it, which was ſufficient to produce the effect, when the ligature was at ſome diſtance from the muſcle.—I ſhould explain this effect in the following manner: By the ligature the nerve is rendered a more imperfect conductor, becauſe the continuation of its medullary part is, in a great meaſure, if not intirely, interrupted. Now when the ligature of the nerve is in contact with the muſcle, the electricity which is ſent through the nerve will run along its ſubſtance as far as the ligature; but meeting in that place with the muſcle, which is a better conductor than the tied part of the nerve, it will run over the ſurface of the muſcle to the other extremity of the conducting rod, and of courſe it will not produce any movements, or very ſlight ones; whereas, when the ligature is at ſome diſtance from the muſcles, though the tied part of the nerve is not ſo good a conductor as the reſt of it, yet the electricity,

tricity, finding no better conductor at hand, muſt neceſſarily paſs through it, and then, being conducted through the ramifications of the nerve, is communicated to all the internal parts of the muſcles, and hence it produces much more powerful contractions.

38. The Animal Electricity is much more eaſily weakened by obſtructing the circulation of the blood, than by interrupting the nervous communication. Thus, if the ſciatic nerve of a living frog be divided, and the crural artery of another living frog, or the other leg of the ſame frog, be tied faſt, ſo as to ſtop the circulation through it, and if, ſome hours or days after this, the legs be prepared in the above deſcribed manner, the contractions excited by the metals in that leg, whoſe artery has been tied, will be found to be weaker and to vaniſh much ſooner than in the other leg, the nerve of which had been divided. Dr. FOWLER, to whom we are indebted for this curious obſervation, in order to prove as a general propoſition, "that the ſanguiferous ſyſtem contributes more im-

"mediately

" mediately than the brain to the ſupport " of that condition of muſcles and of " nerves, upon which the phenomena of " contraction depend," made ſeveral well-contrived experiments, for which the preparations generally conſiſted in evacuating the blood as much as poſſible both from living, and from recently dead, frogs; and then comparing them with other frogs ſimilarly prepared in every other reſpect, except that they were not deprived of their blood. The effect was, that by the application of metals, or other ſtimulus, the former ſhewed weaker contractions, and ceaſed to contract much ſooner, than the latter.

39. Amongſt the other experiments I made with Dr. LIND, we had the curioſity of trying whether the communication of Animal Electricity might be interrupted by ſurrounding that part of the nerve which is between the armour and the leg with white wax; but on applying the conducting rod, the effect was found to be the ſame as when the nerve was not enveloped in wax.

40. It has been ſaid, that it is not abſolutely neceſſary to form a communication between the nerve and the muſcle in order to produce the movements; it being ſufficient to form the communication between two parts of the ſame nerve, or between two muſcles, or even between two parts of the ſame muſcle; one of thoſe parts at leaſt being armed with metal. The effect, however, is much greater when the communication is made between nerve and muſcle. Mr. VOLTA performs the experiment in the following manner: He faſtens to a pair of pincers the crural nerve of a dog or lamb, a little above its inſertion into the leg; the other extremity of the nerve, being ſeparated from the body, is kept ſuſpended by a thread, or is reſted on ſealing-wax. At about one eighth part of an inch above the pincers he lays a piece of money or other metallic plate upon the ſame nerve; then, on paſſing an exceedingly weak charge of the Leyden phial through that part of the nerve, viz. from the pincers to the other metal, or, without any Artificial Electricity, on compleating the communication between the two metals, a con-

a convulſion and agitation of the whole leg will take place; though in this caſe the leg is out of the circuit. But it has been obſerved by Dr. FOWLER, that when the nerve has been wiped dry, or is ſuffered to remain ſuſpended 'till its moiſture is evaporated, this experiment will not ſucceed; it ſeems therefore that the moiſture, which ſurrounds the nerve, may form the communication to and from the muſcle; and of courſe that a communication from nerve to muſcle ſeems to be abſolutely neceſſary for the ſucceſs of the experiment. In addition to the explanation of Mr. VOLTA's experiment we may obſerve, that though the direction of the Animal or Artificial Electricity is determined through the nerve alone; yet part of it may very probably go through the nerve to the muſcle, and may thence return to the nerve; thus, when a man keeps his hand upon a charged Leyden phial, on making the diſcharge of the ſaid phial, by the uſual application of the diſcharging rod to the knob, and to the outſide of it, the man will feel ſome effect of the diſcharge though he is out of the circuit of communication.—When the com-

communication is made between mufcle and mufcle, or between oppofite parts of the fame mufcle, we may naturally remark that it is hardly poffible to avoid touching a nerve; confidering how much the nerves are difperfed and ramified through the muf-cular fibres.

41. The two legs of a frog being fe-parated from the reft of the body, and fkinned, the nerve of one thigh was difen-gaged from the mufcles, and was left at-tached to the leg. Its extremity was coat-ed. When the communication was made by means of the conducting rod between this coating and the middle between the two legs, both legs moved; but the mo-tion of the oppofite leg and thigh was very weak, in comparifon with the motion of the leg to which the armed nerve be-longed.

42. If a nerve be detached from the fpine, and be left attached to the mufcle, on applying the conducting rod to the ar-mour of the nerve and mufcle, a contrac-tion of the depending mufcles only will take place; but, fays Dr. GALVANI, if the nerve

nerve be laid bare and not detached, then on forming the communication the whole body will be convulſed. This laſt obſervation, however, ſeems to be not generally true; the motions being almoſt always confined to the depending muſcles; except when the experiment is tried with a living frog. But in this caſe the Animal Electricity is ſo powerful, as very eaſily to put the whole animal in motion.

43. It has been ſaid, though I doubt the fact, that a ſmall muſcle, being ſeparated from the body of a frog, was placed upon a ſilver coin. On touching it with the ſciſſars in that ſituation, it ſhrunk up; but after this firſt contact, it moved whenever the ſciſſars were only preſented to it, without any actual contact. —In another muſcle, ſimilarly prepared, no motion could be excited by any mechanical ſtimulus; yet the application of the conducting rod cauſed it to move.

44. I took, ſays Dr. VALLI, a frog, which I diveſted of its integuments. I laid bare the ſpine, and divided it above the origin of the crural nerves, and alſo at the

origin

origin of the lower extremities. Thus the frog was in two parts, communicating only by the crural nerves. Thefe nerves I coated; and upon placing one of the branches of the conducting rod on the coating, and the other on the trunk, the lower extremities were inftantly agitated as well as the upper part and fore-legs.—If the experiment be repeated when the nerve is tied, then no motion will take place in the lower extremities. If, inftead of placing the conducting rod on the trunk, it be placed on the ovaries, liver, lungs, head, or fore-legs, the experiment anfwers equally well.

45. Prepare two frogs in the ufual manner. Let the legs of one touch thofe of the other, and let the armed nerves remain at a diftance from each other. Make the communication between the two armours of the nerves, and ftrong motions will be excited in the legs. This experiment feems to fhew a double current of the Animal Electricity.—When the power of one of the prepared frogs is exhaufted, then, on making the communication, the vigorous frog alone will move.

46. The

46. The application of Artificial Electricity will generally excite motions in thofe prepared frogs, whofe Animal Electricity has been exhaufted by the repeated application of the conducting rod. It is very remarkable, that the application of Artificial Electricity has fometimes revived in a great meafure the Animal Electricity, fo that afterwards motions could be again excited by the application of the conducting rod.

47. By the application of armours of different metallic fubftances, and forming a communication between them, the motions may be excited even in an entire living frog, and likewife in fome other living animals, particularly eels and flounders. The experiment is performed thus: A living frog is placed upon a piece of zinc, with a flip of tinfoil pafted upon its back. This done, whenever the communication is formed between thofe two armours, efpecially when filver is ufed, the fpafmodic convulfions are excited not only in the mufcles which touch the metals, but alfo in the neighbouring ones. The

flip

ſlip of tinfoil may be omitted when ſilver is uſed for the conducting rod. The experiment may be performed entirely under water.

48. This experiment may be made with a flounder in a ſimilar, eaſy, and harmleſs manner. Take a living flounder, ſuch as can almoſt always be found at the fiſhmongers, lay it flat into a pewter-plate, or upon a ſheet of tinfoil, and put a piece of ſilver, as a ſhilling, a crown piece, or the like, upon the fiſh. Then, by means of a piece of metal, complete the communication between the pewter-plate or tinfoil and the ſilver piece, on doing which the animal will give evident tokens of being affected.—The fiſh may afterwards be replaced in the water to preſerve it for farther uſe.

49. Excepting frogs and the above-mentioned fiſhes, this experiment will hardly ſucceed with other living animals, unleſs part of the ſkin be removed. A lizard or a mouſe, for inſtance, being faſtened to a table by means of pins, or

otherwiſe, an inciſion muſt be made on its back as far as the fleſh, and a piece of tin-foil muſt be applied to it. A ſimilar inciſion muſt be made in another part, as the thigh or leg, and a piece of ſilver muſt be applied to it. Things being thus prepared, whenever the uſual communication is formed between the two metals, the convulſions are excited, which, *cæteris paribus*, are ſtronger or weaker as the inciſions happen to be made nearer to, or farther from, ſome principal nerve. And for the ſame reaſon, if in this experiment a nerve happen to be laid bare, and the metal be put in contact with it, the uſual metallic communication will be attended with more violent movements. With inſects that have a very dry outſide, the inciſions muſt be made very deep *.

50. It

* "Après avoir tranché la tête à la mouche, au pa-
" pillon, au ſcarabé, &c. je leur fend, tout au long, le
" corcelet avec un canif, ou les petits ciſeaux; et j'in-
" troduis profondement dans la fente, près du cou, un
" morceau de feuille d'etain, (le papier dit impropre-
" ment argenté eſt très à propos) et un peu au deſſous
" j'introduis, de même bien avant dans l'intérieur, le
" tranchant d'une lame d'argent, ou d'une petite mon-
" noye:

50. It often happens in thoſe experiments, and eſpecially when performed with frogs and chickens, that the metallic application cannot excite any motions in the prepared limb, which however can be freely moved by the will of the animal. And, on the contrary, at other times the application of the conducting rod excites motions in limbs, which the animal ſeems to have no power of moving. Thus the application of opium to a muſcle, or to a nerve, ſtops the voluntary motions of the muſcle or muſcles depending on that nerve, yet the application of the armours and metallic rod of communication, will produce motions in them.—There ſeems evidently to be in the animal frame a power of counteracting in a great meaſure the action, whatever that may be, of the metallic application. — When the animal is vigorous and upon his guard, the contrac-

"noye: alors, quand j'avance celle-ci juſqu'au contact "de la feuille d'etain, les jambes commencent à ſe plier, "à ſe debattre, et les autres parties, et le tronc même, "à s'agiter. Il eſt fort amuſant d'exciter de cette ma-"nière le chant d'une cigale," &c. VOLTA's Experiments in the Phil. Tranſactions for the year 1793, p. 33.

tions can ſeldom be excited by this means; whereas, when a part of the body has been rendered previouſly more ſenſible by irritation, ſcarification, &c. then the application of the metals is attended with more conſiderable effects.

51. Even the living human body can be rendered ſenſible of the action of metallic applications, and both the ſenſes of taſte and ſight may be excited by it. Let a man lay a piece of metal upon his tongue, and a piece of ſome other metal under the tongue, on forming the communication between thoſe two metals, either by bringing their edges into contact, or by the interpoſition of ſome other piece of metal, he will perceive a peculiar ſenſation, a kind of cool and ſubacid taſte, not exactly like, and yet not much different from, that produced by artificial electricity. The metals which anſwer beſt for this experiment are ſilver and zinc, or gold and zinc. The ſenſation ſeems to be more diſtinct when the metals are of the uſual temperature of the tongue. The ſilver or gold may be applied to any other part of the mouth, to the noſtrils, the ear,

ear, and other ſenſible parts of the body; whilſt the zinc is applied to the tongue, and on making the communication between the two metals, the taſte is perceived on the tongue. The effect is more remarkable when the zinc touches the tongue in a ſmall part, and the ſilver in a great portion of its ſurface, than *vice verſa.*—Inſtead of the tongue, the two metals may likewiſe be placed in contact with the roof of the mouth as far back as poſſible, and on completing the communication between them, a ſtrong taſte or irritation is perceived. Mr. John Robiſon, in a letter to Dr. Fowler, gives the following curious obſervations: "I had, *ſays he*, a number "of pieces of zinc made of the ſize of a "ſhilling, and made them up into a rou-"leau, with as many ſhillings. I find that "this alteration, in ſome circumſtances, "increaſes conſiderably the irritation, and "expect on ſome ſuch principle to produce "a ſtill greater increaſe. If the ſide of "the rouleau be applied to the tongue, ſo "that all the pieces are touched by it, the "irritation is very ſtrong and diſagreeable. "This explains what I have often obſerv-

" ed, the ſtrong taſte of ſoldered ſeams of
" metal. I can now perceive ſeams in
" braſs and copper veſſels by the tongue,
" which the eye cannot diſcover, and can
" diſtinguiſh the baſe mixtures which
" abound in gold and ſilver trinkets."

And farther on he ſubjoins the following paragraphs: " Put a plate of zinc into one
" cheek, and a plate of ſilver (a crown
" piece) into the other, at a little diſtance
" from each other. Apply the cheeks to
" them as extenſively as poſſible. Thruſt
" in a rod of zinc between the zinc and the
" cheek, and a rod of ſilver between the
" ſilver and the other cheek. Bring their
" outer ends ſlowly into contact, and a
" ſmart convulſive twitch will be felt in
" the parts of the gums ſituated between
" them, accompanied by bright flaſhes in
" the eyes, and theſe will be diſtinctly
" perceived before contact, and a ſecond
" time on ſeparating the ends of the rods,
" or when they have again attained what
" may be called the ſtriking diſtance. If
" the rods be altered, no effect whatever is
" produced.

" Care

"Care muſt be taken not to preſs the "pieces hard to the gums; this either "hinders us from perceiving the convul-"ſion, or prevents it. I find too, that one "rod, whether zinc or ſilver, is ſufficient "for the communication, and even bring-"ing the two pieces together will do as "well, or perhaps better, but the rods are "eaſier in the management."

52. Different perſons are variouſly affect-ed in this experiment; with ſome the ſenſation, or taſte, is either very ſlightly, or not at all perceived, whilſt with others it is very ſtrong and even diſagreeable. Some think it a mere pungency, and not properly a taſte. Mr. VOLTA ſays, he perceives a ſort of acrid, or ſomewhat an alkaline taſte, when the ſilver is applied to the apex, and the tin to the back of the tongue; but when the tin is applied to the apex, and the ſilver to the back of the tongue, then he perceives the ſubacid taſte. Opium, ſpi-rits, and other things which diminiſh the ſenſibility of the tongue, will of courſe render it leſs ſuſceptible of the action of metallic applications.

53. The communication between the two metals may be made various other ways, ſome of which may be more pleaſant and ſatisfactory; place, for inſtance, two large glaſſes full of water contiguous to each other, but ſo as not to touch; put an oblong piece of tinfoil with one extremity into the water of one glaſs, and with the other extremity projecting out of it; —in the water of the other glaſs put one end of an oblong piece of ſilver, and let the projecting parts of thoſe two metals touch each other; then dip the extremity of the tongue in the water of the firſt glaſs, and dip the fingers of one hand into the water of the ſecond glaſs, on doing which, the ſubacid taſte will be perceived, and will continue as long as the fingers and the tongue are kept in that ſituation.

54. In order to affect the ſenſe of ſight by means of metals, let a man in a dark place put a ſlip of tinfoil upon the bulb of one of his eyes, and let him put a piece of ſilver, as a ſpoon or the like, in his mouth. On completing the communication between the ſpoon and the tinfoil, a faint

ſlaſh

flaſh of white light will appear before his eyes. This experiment may be performed in a more convenient manner, by placing a piece of zinc between the upper lip and the gums as high up as poſſible, and a ſilver piece of money upon the tongue, or elſe by putting a piece of ſilver high up in one of the noſtrils, and a piece of gold or zinc in contact with the upper part of the tongue, for in either of thoſe caſes the flaſh of light will appear whenever the two metals are made to communicate, either by the immediate contact of their edges, or by the interpoſition of other good Conductors of Animal Electricity.

55. When this experiment is performed by a perſon having his eyes open in a place in which there is very little light, a by-ſtander will generally perceive a ſlight contraction of the pupil whenever the metals are brought into mutual contact. The experiment will likewiſe anſwer, when one of the metals is placed between the gums and upper lip, whilſt the other metal is placed between the gums and lower lip; but in this caſe the flaſh appears to be diffuſed over the

the whole face, whereas in the other cases, it is confined to the eyes alone. By continuing the contact of the two metals, the appearance of light is not continued, it being only visible at the moment of making the contact, and very seldom when they are separated; it may therefore be repeated at pleasure by disjoining, and again connecting the two metals. When the eyes are in a state of inflammation, then the appearance of light is much stronger. This phenomenon is not alike perceived by every person, some being hardly sensible of it, whilst others observe a very strong flash.

56. In performing both the above-mentioned experiments, viz. that which produces the taste, and the other which produces the flash of light, some persons imagine to feel a gentle warmth diffuse itself over the tongue from its root to the very apex.

57. Besides the sight and taste, no other sense of the living human body has been affected by the application of different metals.—

tals *.—It is neceſſary to obſerve, that in performing experiments with living animals, the various ſtate and diſpoſition of their bodies, produces a great variety of reſults; eſpecially with living frogs, the effects are not always proportionate to the apparent ſtrength and vigour of the animal, and ſometimes they are even in the inverſe proportion of it. Some perſons have had pains produced by the application of metals into their mouths or ears. "After "performing, *ſays Dr. Monro*, this experiment repeatedly, I conſtantly felt a pain "in my upper jaw at the place to which "the zinc had been applied, which conti- "nued for an hour or more; and in one "experiment, after I had applied a blunt "probe of zinc to the Septum Narium, "and repeatedly touched with it a crown- "piece of ſilver applied to the tongue, and "thereby produced the appearance of a "flaſh, ſeveral drops of blood fell from the

* The application of different metals has been tried with perſons that have undergone chirurgical operations, when a nerve has been laid bare, and in that caſe the contractions have been found to take place as in other animals.

"noſtrils;

"noſtrils; and Dr. Fowler, after making "ſuch an experiment on his ears, obſerved "a ſimilar effect *."

58. It has been ſaid, that moſt of the experiments mentioned in the preceding pages, eſpecially thoſe with living frogs, ſucceed beſt when ſurrounded by fewer perſons, than when they are performed in a crowd. This obſervation ſeems to indicate, that the circumambient air may be concerned in it, but the following experiment ſeems to evince the contrary. The legs of a frog, prepared in the uſual manner, were placed entirely under water in a clean glaſs veſſel, and ſome oil was poured upon the water ſo as to cover the whole ſurface of it; the conducting rod was alſo plunged into the water, and was moved by means of a ſtick of ſealing-wax, which paſſed through the water and oil. Though in this diſpoſition the conducting rod and prepared legs were entirely under water, and the communication with the air was cut off by means of the oil and glaſs veſſel, yet

* Experiments on the Nervous Syſtem, p. 26.

whenever

whenever the conducting rod was pushed so as to touch the armour of the nerves and the muscles, the convulsions happened in all appearance as well as when the experiment is performed in air. Dr. GALVANI tried the same experiment in oil, but the movements did not take place *. This failure might have been expected, considering that oil is not a conductor of this power, and that the above-mentioned experiments will not answer, even when the slightest film of oil happens to be interposed between the bodies which form the communication from the nerve to the muscle of a prepared limb.

59. Frogs killed in hot water, even when the water was not hotter than 106°, and then prepared, &c. have been found to retain a small portion of Animal Electricity, which however was of very short duration. Their power is not diminished by being killed in freezing water, or by the preparation being laid upon ice for some hours.

* De Viribus El. in m. m. p. 22.

60. Chickens and rabbits killed by drowning, and afterwards expoſed to the action of metals, by applying the conducting rod to the muſcles, and to a nerve previouſly laid bare and armed, have ſhewn various effects. In ſome every principle of motion was extinct, others ſhewed weak motions. Sometimes the convulſions were pretty ſtrong, though not of long duration; and in ſome inſtances, by the excitation of thoſe motions, the animal has been actually reſtored to life. It has been likewiſe obſerved in other inſtances, that animals which were almoſt dead, have been revived by exciting this influence.

61. Frogs killed by an electric ſhock, that is juſt ſufficient to deprive them of life, and then prepared in the uſual manner, are ſuſceptible of the motions; but when they are killed by means of very ſtrong ſhocks, and alſo when a very ſtrong electric ſhock is ſent through the nerve and muſcle of the prepared limb of a frog, the motions will no longer appear.

62. Frogs have been killed by laying bare the brain, and irritating it, or by applying opium to it. Frogs have been ſtupified and rendered inſenſible of torture by the application of ſnuff. Dogs have been killed by means of hemlock and of arſenic. Frogs and ſome other animals have been killed by being confined in inflammable air, or nitrous air, or dephlogiſticated air. Lizards have been poiſoned with tobacco, and have died in convulſions. But in none of thoſe inſtances the Animal Electricity was deſtroyed.—In the animals killed by confinement in the above-mentioned elaſtic fluids, the motions were very weak, and took place at great intervals of time.

63. Dr. FOWLER made the following curious experiment with opium. He made a tight ligature round the ſciatic nerve of a frog, alſo divided the ſciatic nerve of another frog, and then applied opium to their brains. After this preparation, he excited the motions in their legs by the uſual application of the metals, and found that the leg, whoſe nerve had been tied or

divided, continued to be contracted for a much longer time than the other leg.

64. Air vitiated by the combuftion of fulphur, diminifhes the effects of Animal Electricity, but in a lefs degree when the prepared frog is expofed to it, than when the living one is confined and fuffered to die in it. In the latter cafe, the mufcular fibres fometimes become lax and foft; in both cafes the motions are weak, and vanifh very foon.

65. The moving power of the prepared legs of a frog, is much diminifhed by being kept in a veffel of nitrous air for a certain time, and is entirely deftroyed by a longer continuance in that elaftic fluid.—Inflammable air acts in a fimilar manner, though not fo powerfully. In thofe cafes the mufcles do not appear to have fuffered any alteration.—Perhaps the nerves alone are affected by it.

66. Animals ftarved to death, or killed by means of corrofive fublimate, and afterwards

wards prepared and ſubjected to the action of metals, have ſhewn no motion whatever.

67. The facts which we have noticed above, ſhew, that in animals dead as well as living, the faculty of being put in motion, of being convulſed, &c. by the application of metals and other bodies, poſſeſſes but one characteriſtical property in common with electricity; viz. its being conducted by certain bodies, and not by others. Upon the whole, the Conductors of the one are alſo Conductors of the other; yet this law is not without ſome remarkable exceptions. The convulſion occaſioned by the application of metals, is indeed analogous to the electric ſhock, but that convulſion is likewiſe the effect, though in a more limited manner, of other ſtimuli. The other two peculiar properties of electricity, namely, the light, and the attraction and repulſion, have not been diſcovered in the muſcular power or Animal Electricity of living or dead animals. Upon the ſuppoſition that it is the ſame thing as electricity, the want of light may perhaps be attributed

 to

to its very rarified ſtate and ſmall quantity. But with reſpect to the attraction and repulſion, aſſertions have been publiſhed of thoſe properties having been actually diſcovered; I believe, however, that thoſe appearances of attraction and repulſion muſt be attributed to other cauſes, as the following paragraphs will ſhew. It has been ſaid, that fourteen frogs having been prepared in the uſual manner, the armours of all their crural nerves were connected together, and the ſame thing was done with the muſcles of all their legs; the communication was then made between thoſe two armours, viz. that which communicated with the muſcles, and that which communicated with all the nerves; in doing which two bits of ſtraw, which happened to lie near the circuit of communication, were attracted by it.—*Le Journal de Phyſique* likewiſe mentions, but without deſcribing the method, that undoubted proofs of repulſion, occaſioned by Animal Electricity, had been obſerved with an electrometer. I alſo find recorded, that the hair of a mouſe prepared for thoſe experiments, was obſerved to move whenever a Conductor was preſented to it,

and

and besides, that it moved when situated near the armour of a combination, or battery, of several prepared frogs *.

68. With respect to this last observation, whoever is conversant with electrical experiments, will naturally remark, that the hair of certain animals is so easily electrified by the slightest friction, and continues so long in that state, that the above-mentioned appearances of attraction or motion, may with much more propriety be attributed to the common than to the Animal Electricity. But the other assertions being more positive and less equivocal, induced Dr. LIND and myself to put the subject to the test of actual experiments.

69. For this purpose we prepared six frogs in the usual manner, and laid their legs all parallel upon silver plates, which rested upon a pane of glass. A silver wire was placed in contact with the tinfoil armours of all their crural nerves, and this wire was raised above the glass by means of

* VALLI's Exp. on An. El. p. 112.

ſealing wax. With this preparation, the completion of the communication between the two armours was formed various ways. We placed a pendulum of gold leaf very near the circuit; we placed the pendulum itſelf, which was exceedingly ſenſible, in the circuit of communication; one of the metallic Conductors coming within about one-fiftieth part of an inch of its extremity; we alſo employed a very ſenſible electrometer, diſpoſing it in various ſituations: but in none of thoſe caſes could we diſcover the ſmalleſt ſign of attraction or repulſion; neither could the power be tranſmitted when the ſmalleſt interruption exiſted in the circuit of communication.—We made a cut with a pen-knife acroſs a piece of tinfoil, faſtened upon a ſtick of ſealing-wax. This interruption could hardly be ſo great as the 200th part of an inch, and certainly it was not greater. We attempted to form the communication by means of this apparatus in a very dark room, in hopes of diſcovering the ſpark, but no light whatever could be perceived, and indeed it could hardly be expected, conſidering that this ſmall interruption was quite ſufficient to prevent

prevent the motions, or the communication of that power, which, for want of a better name, we call Animal Electricity.

70. The principal phenomena of Animal Electricity, viz. the property of being put in motion by a metallic or other communication made between the nerves and the muscles, is not peculiar to a few animals only, but seems to be a property of all animals in general; a law of nature, which admits of few exceptions, and even those exceptions are of a very doubtful nature. The experiments have already been tried with a great variety of terrestrial, aerial, and aquatic animals. The human body, whilst undergoing certain chirurgical operations, or its recently amputated limbs, have been convulsed by the application of metals. From the ox and the horse down to the fly, the effects of metallic applications have been repeatedly and unequivocally observed. With some the power lasts longer than with others; the movements also are more or less evident and powerful, according to the various nature and disposition of the animals. The leg of a recently dead horse

was agitated ſo violently by the application of a ſhilling and a bit of tinfoil, that the ſtrength of a robuſt man was unable to check the blow. Animals poſſeſſed of cold blood, are in general more retentive of that power than thoſe which have hot blood; but amongſt thoſe of the ſame claſs a conſiderable variety is obſervable, which ariſes from the different ſtrength or irritability of their fibres, and probably from other cauſes that are as yet unknown. The animals which form an exception to the above-mentioned general law, are ſeveral worms, ſome other inſects, the oyſter, and a few other ſmall ſea animals. But as the organization of thoſe animals ſeems not to be poſſeſſed of much ſenſibility, nor admits of much motion, it may be preſumed that the effects of the metallic application are only too weak to be perceived by our ſenſes; and in fact ſeveral animals, which ſome time ago were thought not to be affected by the contact of metals, have been lately cauſed to contract in conſequence of the diſcovery of more active metallic combinations, or of ſome of their more ſenſible parts.

71. The

71. The preceding pages contain all the remarkable facts that I have been able to collect, relative to a ſubject which is likely to become of great importance. Thoſe ſurpriſing effects of an unknown cauſe, generally inexplicable, and ſometimes contradictory, ſeem to admit of no theory ſufficiently probable or ſatisfactory, nor can we yet ſee how they may be applied for the benefit of mankind. An attentive conſideration of the ſubject will naturally ſuggeſt ſeveral doubts and queries, which can only be anſwered by future experiments and diſcoveries.—In what manner does artificial electricity affect the muſcles?—Does it act as a mere ſtimulus or otherwiſe?—Where is the Animal Electricity generated, and by what mechaniſm is it tranſmitted from one part of the body to another?—Does it proceed from the brain, or is every nerve actuated with that generating power?—What reaſon can there be for the neceſſity of uſing two different metals?—And after all, are thoſe phenomena really the effects of electricity, or of ſome other unknown fluid *ſui generis?*

72. The want of ſeveral of the characteriſtic properties of electricity, may perhaps be owing to the weak ſtate of that power in animals, and therefore it would be unphiloſophical to admit another agent as the cauſe of thoſe muſcular motions, contractions, &c. unleſs a property of it could be diſcovered, which is abſolutely repugnant to the aſcertained laws of electricity. In that caſe we might with propriety ſay, that as there are ſeveral liquids or viſible fluids like water, ſpirits, &c. which have diverſe properties in common, at the ſame time that they are eſſentially different; that as there are ſeveral inviſible and permanently elaſtic fluids like common air, inflammable air, fixed air, &c. which are very diſſimilar, though poſſeſſed of certain common properties; ſo there may be ſeveral ſorts of more ſubtile fluids eſſentially different from each other, yet bearing ſome analogy to the electric fluid.

73. Having, towards the beginning of this account, ſhewn the poſſibility of the electric fluid exiſting in an unbalanced ſtate amongſt the various parts of the animal body, I ſhall conclude

conclude with a few remarks concerning the origin of the accumulation or rarefaction of that fluid in general, which may probably promote the inveſtigation of this curious ſubject.

74. There is a well known and very extenſive law in the ſcience of electricity, which is, that the mere proximity of an electrified body, is ſufficient to induce a contrary electricity in another body, without its loſing any part of its own*. Upon this principle, if the permanent exiſtence of a quantity of electricity in any place be admitted, one may eaſily conceive how other bodies may be electrified by it, and alſo how the electricity may thereby be accumulated to any degree. But it will naturally be aſked, where is that electrified body, the firſt term of the ſeries, from which the accumulation may be derived?—To this I anſwer, that ſtrictly ſpeaking, the common notion of the electric fluid exiſting in a balanced ſtate amongſt the bodies of

* The action of Mr. VOLTA's Electrophorus, and of the Doubler of Electricity, are intirely depending upon this law.

our globe, is by no means true. Great quantities of electricity accumulated on bodies that are not absolutely insulated, will be readily dispersed amongst the surrounding bodies, in the same manner as a quantity of water, which is poured out of a vessel upon any surface, will soon find its level, by descending from the highest to the lowest places. But let a man try to remove the last drops of water, or particle of moisture, from the inverted vessel, and he will find it very difficult to succeed. In like manner those persons, who are accustomed to make nice electrical experiments, know how extremely difficult it is to remove small residuums of electricity from a Leyden phial, from a piece of wood and other bodies, which have been once electrified. It is evident, therefore, that a beginning of electric accumulation is by no means difficult to be found. But, independent of this remark, if we consider that electricity is generated by evaporation, condensation, rarefaction, friction, and other causes; and that those natural processes happen continually and in every place, we must then conclude, that, far from remaining in a balanced or level state,

ſtate, the electric fluid muſt be continually fluctuating amongſt the various ſubſtances of our globe. It is accumulated in ſome and rarefied in others; the accumulation is removed from the latter to the former, and perhaps it ſeldom happens that two bodies of ſimilar ſhape, bulk, and ſubſtance, contain exactly equal quantities of electric fluid. This accumulation and rarefaction of it, this poſitive and negative ſtate, is in moſt caſes too ſmall to affect our electrometers and other inſtruments; but the effects of very ſmall quantities of artificial electricity upon animals, ſhew that it is by no means too ſmall for the mechaniſms framed by the moſt exquiſite hand of Nature.

## HISTORY

### OF THE

## DOUBLER OF ELECTRICITY.

ON the 10th of May, 1787, the description of a new electrical inftrument was read at the Royal Society, and was afterwards publifhed in the 77th vol. of the Philof. Tranf. This inftrument was by its inventor, the Rev. A. Bennet, named a *Doubler of Electricity*, or a machine by which the leaft conceivable quantity of pofitive or negative Electricity may be continually doubled, till it becomes perceptible by common electrometers, or in vifible fparks.

It confifts of three circular brafs plates, A, B, and C, all of the fame diameter, which is about three or four inches. A is polifhed and thinly varnifhed on the upper furface;

surface; B is polished and varnished on both sides, and is furnished with an insulating handle, which is fastened to its circumference; C is polished and varnished on the under side, and is furnished with an insulating handle, that rises perpendicular to the plate from the centre of its upper surface.

The method of doubling, quadrupling, &c. a small quantity of electricity with this instrument, is the following: The plate A is placed horizontally upon an insulating stand, with its varnished surface upwards, and the plate B is laid upon it. The small quantity of electricity which is required to be doubled, whether from the atmosphere, or otherwise produced, must be communicated to the under side of the plate A, and at the same time the plate B must be touched with a finger; in consequence of which, the last-mentioned plate will acquire an electricity contrary to that of the plate A. This done, the plate B must be lifted up by its insulating handle; the third plate C must be laid upon it, and in that situation C is touched with the finger, in conse-

quence of which the plate C will acquire an electricity contrary to that of B, and of course of the same sort with that of A. Now the plates B and C, being held by their insulating handles, must be separated from each other. B must be laid upon A, and must be touched with the finger, whilst C is made to touch with its edge the under side of the plate A. By this means, the original quantity of electricity is doubled once, as may be easily comprehended after a little consideration, and by repeating the above-described operation, it may be doubled again and again to any degree, viz. the plate B being removed from over the plate A, the third plate C is laid upon it, and C is touched with a finger; then these two plates are separated, B is replaced upon A, and is touched with a finger, whilst C is kept in contact with the lower side of it, and so on.—This effect depends upon the two well-known laws of Electricity, viz. first, that a body presented to another body electrified, acquires thereby the contrary electricity, provided it be made to communicate with other bodies or with the earth; and secondly, that the capacity of a body for hold-

ing electricity, is increaſed by being placed with its ſurface contiguous to another body not inſulated.

The advantages to be derived from an inſtrument of this ſort appeared at firſt ſight ſo ſtriking, that I ſoon procured one of them, and uſed it for the diſcovery of ſmall, and otherwiſe imperceptible quantities of electricity; but a few trials ſoon ſhewed, that no dependence was to be placed upon ſuch an inſtrument, the uſe of which was generally, if not always, attended with equivocal reſults. Sometimes it ſhewed different electricities after ſimilar operations, and at other times it ſhewed the very ſame ſort of electricity, when, from the difference of the original communication, it ought to have ſhewn different electricities. But its more remarkable fault was, that in a number of experiments, it generally continued to ſhew that ſame ſort of electricity which had been communicated to it in the firſt experiment.

It naturally occurred to me, that the defect aroſe from the contact of the varniſhed

ſurfaces

ſurfaces of the plates, which might in ſome meaſure electrify them; and from their retaining a portion of that ſort of electricity, which happened to be communicated to them in the firſt experiment. In conſequence of this conſideration, I conſtructed a doubler which could perform the ſame office, but in a leſs exceptionable manner. It conſiſted of three large and flat tin plates not varniſhed, and each of them was ſupported in a perpendicular ſituation on a ſeparate glaſs ſtand. They could be either placed facing each other at any required diſtance, without touching, or could be made to touch each other edgewiſe. With theſe plates A, B, and C, the ſame operation could be performed as with the above deſcribed doubler, viz. A and B were placed facing each other at the diſtance of about one-tenth of an inch, and the original ſmall quantity of electricity was communicated to A, whilſt B was touched with a finger; then B being removed from the vicinity of A, was placed facing the plate C, and in this ſituation C was touched. After this the plate C was ſituated with its edge in contact with the back part of A; B was

B was put facing A, and in that ſituation was touched, &c.

As the plates of this inſtrument were not varniſhed, the poſſibility of exciting any electricity was removed, and in fact the effect ſeemed to be ſomewhat more conſtant, yet it was far from being uniform and certain. It appeared, in ſhort, that without communicating any electricity to it, the inſtrument after doubling a few times became always ſenſibly electrified. When the inſtrument had not been uſed for ſome days, then, in order to make it produce that ſpontaneous ſort of electricity, it was neceſſary to repeat the operation of doubling at leaſt 20 times; but in any ſubſequent trial after this firſt experiment, a manifeſt degree of electricity could be produced by doubling about 12 times, and this electricity was almoſt always of the ſame ſort as in the firſt experiment.

The difficulty now conſiſts, in finding out the origin of that electricity which is produced by this inſtrument, without the apparent communication of any quantity of

it. In my opinion it muſt originate either from the ſurrounding air, or from the inſtrument itſelf, and probably from both cauſes. For inſtance, when the inſtrument has remained a long time unemployed, its plates are then likely to be in the ſame ſtate of electricity, and therefore that plate, which in the beginning of the operation remains untouched, will probably acquire ſome electricity from the ſurrounding air, and of courſe that electricity will be doubled until it becomes manifeſt. But when the plates have thus been once electrified, it is then extremely difficult, if not impoſſible, to deprive them of ſome reſiduum of electricity, which they ſeem to retain even after one or two days reſt; at leaſt I have not been able to ſucceed in the attempt of removing it. The reſiduum adheres probably to that part of the inſulating ſtand which is contiguous to the metal plate, from whence even the flame of a candle is not ſufficient to remove it. To this reſiduum then the origin of the electricity, in the trials after the firſt, muſt be generally attributed. Sometimes the electricity communicated by the atmoſphere, may be much

9 ſtronger

ſtronger than the reſiduum that adheres to the plate, and then of courſe the electricity of the atmoſphere will be manifeſted by the operation of doubling; but as the quantity of reſiduum, or adheſive electricity, is variable, as well as the electricity of the ſurrounding air, or of any other proceſs under conſideration, therefore one can never be ſure of the effects reſulting from the uſe of ſuch an inſtrument.

My objections to the uſe of the Doubler, even when made in the above-mentioned leſs exceptionable manner, were read at the Royal Society in November, 1787, and were afterwards publiſhed in the 78th vol. of the Phil. Tranſ. On the 5th of June following, the deſcription of an improved Doubler by Mr. WM. NICHOLSON, was read at the Royal Society, and was afterwards publiſhed in the 78th vol. of the Phil. Tranſ. Part II.—Mr. NICHOLSON adapted the three plates A, B, C, to a mechaniſm, in which nothing more was required to be done than to turn a winch; for by the very ingenious conſtruction of its parts, in every revolution, the whole operation of once

doubling was accompliſhed, a ſecond revolution quadrupled the electricity, and ſo on. In this machine the plates do not touch one another, but only come with their ſurfaces very near each other, exactly as I had done with the three ſeparate plates. Mr. NICHOLSON very properly calls this machine, "an inſtrument, which, by the turning of "a winch, produces the two ſtates of elec- "tricity without friction, or communica- "tion with the earth." He acknowledges the propriety of my ſuppoſition, concerning the origin of the electricity in this inſtrument, viz. that it is generally owing to ſome reſiduum adhering to its parts, and concludes with the following juſt obſervation: "But here, as well as in the common "Doubler, the effect is rendered uncertain "by the condition, that the communicated "electricity muſt be ſtrong enough to de- "ſtroy and predominate over any other elec- "tricity the plates may poſſeſs. I ſcarcely "need obſerve, that if this difficulty ſhould "hereafter be removed, the inſtrument will "have great advantages as a multiplier of "electricity in the facility of its uſe, the very "ſpeedy

"ſpeedy manner of its operation, and the "unequivocal nature of its reſults."

Mr. BENNET, in his New Experiments on Electricity, publiſhed in 1789, page 84, deſcribes a method of depriving the Doubler of its adheſive or ſpontaneous electricity. It conſiſts in turning the handle round a conſiderable number of times, whilſt all the three plates of the inſtrument are communicating with the ground by means of wires. But I muſt confeſs that I could not upon trial, find this method ſufficient to produce the deſired effect.

Notwithſtanding thoſe objections, ſeveral diſcoveries are ſaid to have been lately made with it, the moſt remarkable of which are the following; and the Doubler is at preſent by ſome perſons, conſidered as one of the moſt uſeful inſtruments in electricity.

In a paper of Mr. J. READ, publiſhed in the Phil. Tranſ. of the preſent year 1794, ſeveral experiments are related, which concur to eſtabliſh the following general pro-

position, viz. that common atmospheric air, when vitiated even in a small degree by respiration, putrefaction, and other natural processes, shews a deficiency of electric fluid; whereas in its natural pure state, it is generally possessed of positive electricity. The facts upon which Mr. READ establishes this proposition, were proved by means of the Doubler; for when he worked it in a close room, in which people had been shut up for a certain time, or upon a dunghill, or in a privy, he obtained negative electricity; but when he worked it in the open and clear air, the result was positive electricity.

" Knightsbridge charity-school, *says he in* " *page* 269, fills up a piece of ground be-" tween the north-end of the chapel and " Hyde Park wall, and the main sewer of " that neighbourhood runs at no great " depth under it; the number of children " educated in this school, is thought by " some to be too great for the size of the " school; on these accounts it becomes in-" fected with a very disagreeable stench, " especially when the door and windows " are

" are ſhut up; I have ſometimes found the
" noxious effluvium ſo very ſtrong in this
" ſchool, that I have haſtened out to breathe
" a purer air. I have often examined the
" electrical ſtate of the air in this ſchool
" with the Doubler, and have always found
" it ſtrongly negative; which ſhewed that
" the aqueous or other conducting matter
" lodged in the air of the ſchool, poſſeſſed
" leſs than their natural quantity of elec-
" tricity, while that of the ſchool-maſter's
" parlour adjoining, having nobody in it,
" poſſeſſed ſomewhat more than its natural
" quantity; it was found therefore poſitively
" electrified.

" July 5th, therm. 76°, I went to the
" ſchool, and found the door and windows
" ſet wide open to let in cool air; I now
" perceived no ſtench at all in the ſchool,
" and thought it needleſs to try it. How-
" ever the ſchool-maſter obſerved, that the
" further end of the ſchool was at all times
" moſt infected with, and ſeldom quite clear
" of ſtench. I therefore worked the Dou-
" bler in that part of it, and after a very
" few turns it became electrified negatively,
" rather againſt my expectation. I then

"tried the other end of the ſchool, which, "by the door being wide open, was leſs "liable to retain any noxious effluvium, "and there the Doubler gave poſitive elec-"tricity. After this I tried it in the ſchool-"maſter's parlour, where it was poſitive "alſo."

Upon a dunghill, Mr. READ made the following experiments: "I firſt, *ſays he*, "went upon the higheſt part of the dung-"hill, and held the Doubler in one hand a "little above the dung, yet within the reach "of the aſcending pale-coloured vapour, "and with the other I turned the revolving "plate five or ſix times, by which means "the Doubler became electrified poſitively; "which was known to be at that time the "general electrical ſtate of the atmoſphere. "—The Doubler was now lowered, and "placed upon the dung; and alſo it be-"came charged by a few turns of the re-"volving plate, which made me ſuſpect "that I had not completely cleared the "Doubler of its former charge; however, "on trial it was found to be electrified ne-"gatively.

"I re-

"I repeated theſe experiments with the "Doubler, placed on various parts of the "dunghill, without any remarkable variation "in the reſults, except that the negative ſtate "of the exhaled vapour was ſtronger, and "extended to a greater diſtance, in the de- "preſſed or hollow parts of the dung, where "the vapour was leſs expoſed to a mode- "rate gale of wind that then prevailed, than "on the open parts which lay full to it."

In the additions at the end of Dr. DARWIN's Zoonomia, I find the following paſſage: "By the experiments publiſhed by "Mr. BENNET, with his ingenious Dou- "bler of Electricity, which is the greateſt "diſcovery made in that ſcience ſince the "coated jar, and the eduction of lightning "from the ſkies, it appears, that zinc was "always found minus, and ſilver was al- "ways found plus, when both of them "were in their ſeparate ſtate."

Not knowing of any other experiments publiſhed by Mr. BENNET on this ſubject, I imagine that the above-mentioned paragraph alludes to the experiments related in Section

Section VII. of Mr. BENNET's New Experiments, &c. The title of this section, is "Experiments on the adhesive Electricity of "Metals, and other conducting Substances." Those experiments are performed in the following manner: The Doubler is first of all deprived of its natural electricity, as far as that object can be obtained, by turning the handle about 50 times round whilst the plates communicate with the ground. The communication is then removed, and whilst the revolving plate stands opposite to one of the fixed plates, two different metallic substances are applied, one to the former, and the other to the latter of those plates. The handle being then turned round, after a various number of revolutions, the instrument becomes electrified sufficiently to affect an electrometer. The experiment is also performed, by applying a single substance to one of the plates. Mr. BENNET found that the electricity produced was different, according to the metal that was applied. But from the account of his own experiments, and much more from the experiments of other persons, the results are not constant.

Of

## Of the METHODS of

### Manifesting the PRESENCE, and ascertaining the QUALITY, of small Quantities of

## Natural or Artificial ELECTRICITY.

THE persevering industry of many ingenious persons in the philosophical world, since the beginning of the present century, has gradually, and with considerable success, examined the effects, ascertained the properties, and investigated the laws of a science almost entirely unknown before. Various wonderful phenomena of nature have been explained, means have been discovered of avoiding some of their pernicious effects, and a greater insight into the general operations of nature, has been derived from the knowledge of so great and so extensive a power as is commonly understood under the name of *Electricity*; yet, if instead of contemplating its more striking

ing phenomena, viz. thoſe which are apt to ſurprize more by the magnitude of their effects, than by the inſtruction which they may afford, we endeavour to examine the ſubject more cloſely, we ſhall find that our knowledge of Electricity goes very little, if at all, beyond the ſuperficial part of it. We know, for inſtance, that a piece of glaſs, or other electric when rubbed, will produce that power called Electricity, that the glaſs will communicate the acquired electricity to a piece of metal, that the piece of metal will retain that power in certain circumſtances, and ſo on; but no perſon has ſhewn how ſuch power is generated by the friction, or what prevents its paſſage through the ſubſtance of ſome particular bodies. It has been aſcertained, that the air of moſt countries, and probably of the whole world, as well as the clouds, fogs, rain, &c. are almoſt always electrified; but we are ignorant of the office which this electricity can have in the great laboratory of nature; for ſurely ſo great and ſo active a power, can hardly be intended by the great Author of the univerſe, merely to intimidate mankind with the thunder and the lightning.

lightning. Thofe perfons, therefore, who are now willing to diftinguifh themfelves in this interefting branch of natural philofophy, ought to examine the electrical power, not fo much in its accumulated, as in its incipient ftate. If its firft origin could be inveftigated, it would be afterwards very eafy to comprehend its increafe; for we may eafily underftand how a great quantity of it may be accumulated, from the often repeated additions of its fmalleft portions.

This truth having been underftood by feveral philofophical perfons, induced them to contrive inftruments neceffary for the purpofe, and to make numerous obfervations, which have undoubtedly promoted the knowledge of the fubject; but a vaft deal ftill remains to be done in order to afcertain the origin, the nature, and the various ufes of electricity.

The various inftruments and methods contrived for this purpofe, may be comprehended under two general heads, viz. 1ft, thofe which fhew the Electricity in its natural

tural quantity and quality; and 2dly, thoſe which manifeſt the electricity in a ſort of unnatural ſtate, as by condenſing, multiplying, or changing it in contrarium. The inſtruments of the firſt claſs are the electrometers, which have been deſcribed in the firſt and ſecond volume of this treatiſe, excepting however the gold-leaf electrometer, which we ſhall deſcribe before we proceed to examine the inſtruments of the ſecond claſs.

This electrometer was invented by the Rev. A. Bennet *. It conſiſts of two ſlips of gold-leaf ſuſpended to the cover of, and hanging within, a cylindrical glaſs veſſel. It is in ſhort like my bottle electrometer, excepting that ſlips of gold-leaf are uſed, inſtead of the corks ſuſpended by fine ſilver wires. The ſlips of gold or ſilver leaf are about three inches long, and ſometimes they are made narrower at the lower than at the upper extremity. This electrometer is certainly the moſt ſenſible inſtrument of the kind, and very uſeful in nice experi-

* See the Phil. Tranſ. vol. 77.

ments,

ments, it being affected by very ſmall quantities of electricity, in a ready and unequivocal manner *, but it is by no means portable.

As it is rather difficult to faſten the ſlips of gold-leaf to this electrometer, and to cauſe them to hang parallel, I have contrived a method which remedies that defect. When the ſlips are cut and are lying upon paper, or on the leather cuſhion upon which they are cut, I make them equal in length, by meaſuring them with a pair of compaſſes, and cutting off a ſuitable portion from the longeſt; I then cut two bits of very fine gilt paper, each about half an inch long, and a quarter of an inch broad, and by means of a little wax, ſtick one of them to one extremity of each ſlip of gold-leaf, ſo as to form a kind of letter T. This done, I hold up in the fingers of one hand, one of thoſe pieces of paper with the gold-leaf

* The electrometers made with cork or pitch balls, are ſubject to an inconvenience, which does not take place in the gold-leaf electrometer; it is, that ſometimes when they are electrified, the balls adhere to each other for a conſiderable time before they will ſeparate, and then they ſeparate with a kind of jirk all at once.

ſuſpended

ſuſpended to it, and hold the other with the fingers of the other hand; then bringing them near to each other, and having adjuſted them properly, viz. ſo as to let them hang parallel and ſmooth, I force the pieces of paper which now touch each other, between the two ſides of a ſort of pincers made of braſs wire, or of very thin and hammered braſs plate, which pincers are faſtened to the under part of that piece which forms the top or cover of the glaſs veſſel. As theſe gold ſlips are very apt to be ſpoiled, I keep ſeveral of them ready cut in a book, each having a croſs piece of paper faſtened to one extremity, ſo that in caſe of accident, a new pair of gold ſlips may be ſoon put between the aperture of the above-deſcribed pincers; and by this means the electrometer is rendered, in a certain manner, portable.

Of the other ſort of inſtruments uſeful for diſcovering the preſence and quality of ſmall quantities of electricity, ſeveral have already been deſcribed in the preceding part of this treatiſe. In pages 41 and 65 of Vol. II. the reader may ſee how a weak

and

and diffuſed electricity may be manifeſted by expoſing an electrometer, or an electrophorus plate to it; for theſe will thereby acquire the contrary electricity agreeably to the laws of electric atmoſphere. In page 259 and following of the ſame vol. is deſcribed Mr. VOLTA's method of condenſing the diffuſed and otherwiſe imperceptible quantity of electricity upon a metal plate, which will afterwards electrify an electrometer very ſenſibly. But it remains now to deſcribe the methods of rendering perceptible a quantity of electricity, which is in itſelf too ſmall either to affect an electrometer or Mr. VOLTA's condenſer; for, in fact, this condenſer is only capable of collecting together a quantity of electricity, which, though not very ſmall, is however ſo little condenſed, or ſo diffuſed through a proportionably large ſpace, as not to be capable of affecting an electrometer: hence, if by means of this apparatus the experimenter expected to render the electricity of a ſmall tourmalin, or that of a hair when rubbed, more manifeſt than it generally is, when communicated immediately to an electrometer, he would find himſelf miſtaken.

The doubler of electricity is the inſtrument which is capable of increaſing a very ſmall quantity of electricity to a degree more than ſufficient to manifeſt its quality; but the properties and defects of that inſtrument have been ſufficiently noticed in the preceding pages. I endeavoured, in conſequence of thoſe obſervations, to conſtruct an inſtrument which might anſwer that purpoſe in an unequivocal manner, and after ſome fruitleſs attempts, I at laſt ſucceeded in the invention of one, which is not ſubject to equivocal reſults. It renders a ſmall quantity of electricity manifeſt, by continually adding portions of the contrary electricity to an inſulated plate, until that plate becomes ſo far charged with it, as to electrify an electrometer, which of courſe will manifeſt an electricity contrary to that originally communicated to the inſtrument. With this machine, which by way of diſtinction we ſhall call the *Multiplier of Electricity*, the accumulation of the communicated power does not advance by any means ſo quick as with the doubler; but the experiments contained in the following

2 pages

pages will convey a much better idea of its performance.

The figures 3d and 4th reprefent this new inftrument, and they are about two-thirds of the real fize. QRS is the bottom board, upon which are fteadily fixed on the glafs fticks H, G, two flat brafs plates A and C.—B is a fimilar brafs plate fupported by a glafs ftick I, which is cemented in a hole made in the wooden lever K L, which moves round a fteady pin K, that is fcrewed tight in the bottom board. By moving this lever backwards and forwards, the plate B may be alternately put in the two fituations reprefented by the figures. N is a thick brafs wire fixed tight into the bottom board. O M is a crooked wire that proceeds from the focket on the back of the plate B.—There is a fourth brafs plate D, fimilar to the other three, which is fupported not by glafs, but a wire; and this wire is fcrewed faft to an oblong piece of brafs F P, that flides in a groove made for the purpofe in the bottom board Q R S; fo that by applying a finger's nail to the notch on the end F, the fliding piece F P

may be drawn out either intirely or to a certain length, and of courſe the plate D will be removed to any required diſtance from the fixed plate C. I need not ſay any thing particular reſpecting the ſockets of thoſe braſs plates, they being clearly indicated in the figures, excepting only that the ſocket of the plate A reaches as high as the top of it, and ſerves to receive a wire, or other apparatus, in certain occaſions *.

The parts of this inſtrument are ſo adjuſted, as that when the lever is in the ſituation of fig. 3d, viz. is puſhed as far toward Q as it can go, the plate B comes parallel to the plate A, and about one-twentieth of an inch diſtant. At the ſame time the extremity of the wire O M juſt touches the fixed wire N, and of courſe renders the plate B un-inſulated. But as ſoon as the lever begins to be moved towards S, the communication of the plate B with the wire

* I have not been particular in mentioning the real dimenſions of the parts of the inſtrument, as being not material. The plates of mine are an inch and a half ſquare; but I think that it would be better if they were a little larger.

 N, or

N, or with the ground, becomes interrupted, and B remains insulated. And when the lever has been moved as far as it can go towards S, the wire M comes in contact with the plate C, as shewn in fig. 4th. Then the two plates B and C communicate with each other, though they are otherwise insulated. The fourth plate D being supported by a wire, communicates with the ground; and when the sliding piece P F is pushed home, it stands parallel to, and at about one-twentieth of an inch from, the plate C.

When the instrument is situated as in fig. 3d, if an electrified body be brought into contact with the plate A, this plate will imbibe a great deal more of that electricity than it would otherwise, because its capacity is increased by the vicinity of the un-insulated plate B, and therefore, if after the communication of that electricity, the plate B, by moving the lever, be removed from that situation, and A be made to touch an electrometer, this will be electrified more sensibly by it, than it would have been by the contact of the original electrified body

itſelf. So far the plate A acts like a condenſer, or collector, of electricity. But let us now conſider the inſtrument as a multiplier.

When the plate A has received a ſmall quantity of electricity by the contact of any electrified body whatever, and that body is removed, the plate B being un-inſulated and oppoſed to the electrified plate A, will, like the metal plate of an electrophorus, acquire the contrary electricity, by either receiving from, or giving to, the ground ſome electric fluid, according as the plate A happens to be electrified. Thus, ſuppoſe that A has been electrified poſitively, B will become negative, and *vice verſa*. If now the lever be puſhed towards S, the plate B will remain electrified negatively, the communication with the ground being cut off; and when B comes into the ſituation repreſented by fig. 4th, at which time the wire M touches the plate C, the negative electricity of B will go to C, becauſe the capacity of C for holding electricity is conſiderably augmented by the vicinity of the un-inſulated plate D. If after this the lever be moved

moved back again to its firſt ſituation, B will be made negative a ſecond time in the ſame manner as before: and by puſhing the lever again towards S, that ſecond charge of negative electricity will be communicated from B to C; and thus, by repeating the operation, which conſiſts in merely moving the lever backwards and forwards, a conſiderable quantity of negative electricity will be accumulated upon C.

In fact, the action of this inſtrument reſembles very much that of an electrophorus; for the plate A may repreſent the excited reſinous plate, B may repreſent the metal plate of the electrophorus, and C is a kind of reſervoir, into which the ſucceſſive charges of the plate B are collected.—When a number of thoſe charges or portions of electricity has been communicated to C, if the ſliding piece F P be drawn out about an inch, and of courſe the plate D be removed to the like diſtance from the plate C, the capacity of the plate C will thereby be much diminiſhed: and therefore if an electrometer be brought into contact with it, the electricity will be ma-

nifested: whereas the electricity originally communicated to the plate A, could not have affected an electrometer in any sensible degree.

In using this instrument, 30 or 40 additions of electricity are the utmost number practicable; for after that number the augmentation of the charge upon C will not go any farther; the limit of which is, when the charge of C is increased to such a degree, as to leave a portion of electricity upon B, equal to that portion which B can receive from the action of A.

In this case, let C touch an electrometer as mentioned above, and if the electrometer does not diverge, proceed to a second process; for though its pendulums do not diverge, yet some electricity remains in them, which must not be disturbed, as it will help the effect of the second operation, which is as follows: Push in the slider F P, and go on moving the lever backwards and forwards as before, by which means, after a certain number of additions, the plate C will acquire a second charge, about as high as the former: and if then the slider F P be

pulled

pulled out, and C be brought into contact with the ſame electrometer, the divergency of the pendulums, which before was either not at all or hardly perceptible, will thereby be rendered more conſpicuous: and thus it may be increaſed ſtill farther by a third and a fourth operation. But if, notwithſtanding thoſe repeated operations, the electrometer ſhould be found not to diverge, the quantity of electricity may ſtill be augmented by another method, which is, by communicating that little electricity of C to the plate A of another inſtrument of the ſame ſort, and proceeding with that in the manner already deſcribed.

The principal cauſe, which renders this inſtrument certain in its effects, is, that all the reſiduum of electricity, which can poſſibly remain upon the plate A, is always too inconſiderable to produce any bad effects; the electricity being never accumulated upon this plate, but conſtantly remaining in its original quantity, or rather in a decreaſing ſtate: whereas in the doubler the electricity is increaſed to a prodigious degree upon the plate that received the original quantity;

tity; and as the reſiduum muſt be greater or ſmaller, according to the charge of the plate, though not exactly in that proportion, it follows, that the reſiduum left upon the receiving plate of the doubler may be actually greater than the quantity of electricity, which was firſt communicated to it.—I ſhall now deſcribe a few experiments made with this multiplier, in order to render both the management of the inſtrument more familiar, and its degree of ſenſibility more evident; previous however to this, it will be neceſſary to remind the reader of two things, viz. firſt, that the electricity communicated by the plate C to the electrometer is contrary to that poſſeſſed by the plate A; and, ſecondly, that the meaning of four, five, or any number of additions, is, that the lever L K has been moved four, five, or any other number of times from the ſituation of fig. the 3d to that of fig. the 4th. But when it is ſaid that four, five, &c. more additions produced a greater divergency, the meaning then is, that after having communicated the firſt charge of C to the electrometer, the plate D was replaced near C, the lever was moved backwards

wards and forwards four or five times more, and then the ſecond charge of C was likewiſe communicated to the ſame electrometer.—It is almoſt ſuperfluous to obſerve, that after having performed one experiment, it is neceſſary to touch with a finger all the three plates A, B, and C, in order to deprive them of the electricity acquired in the operation, and to render the inſtrument fit and ready for a ſecond experiment.

Exp. I. An electrometer was electrified poſitively in a ſmall degree; then time was allowed for that electricity to diſſipate till the pendulums of the electrometer appeared juſt to touch each other, and when this happened, the plate A was brought into contact with the electrometer, ſo as to receive from it part of that ſmall quantity of electricity. This done, the lever was worked, and after ten additions the plate C was found capable of electrifying an electrometer ſufficiently to ſhew, that the electricity was negative. Ten additions more increaſed the divergency of the pendulums, and ten more made them ſtrike againſt the

the glaſs bottle in which they were contained.

Exp. II. About a tea-ſpoonful of water was poured upon a burning coal, and immediately after an inſulated tin plate of eight inches in diameter was held over it for about five or ſix ſeconds of time, to catch and condenſe the vapour. This plate was then brought into contact with the plate A of the multiplier, and after four additions the electrometer was ſenſibly electrified by the contact of the plate C. Four additions more increaſed the divergency of the electrometer to a degree more than ſufficient to ſhew that its electricity was negative, and conſequently that the tin plate had been electrified poſitively by the condenſing vapour.

Exp. III. A burning coal was placed upon the above-mentioned tin plate, and only two drops of water were let fall upon it. The tin plate was brought in contact with the plate A, and after very few additions the plate C electrified the electrometer

ter to a degree much more than ſufficient to ſhew that its electricity was poſitive, and that of courſe the tin plate had been electrified negatively by the evaporation of the two drops of water.

Exp. IV. The above-mentioned tin plate being held by the extremity of its inſulating ſtand, I ſlapped it three times with my open hand. It was then made to touch the plate A of the multiplier.—After thirty additions, the electrometer received from the plate C a ſufficiently ſenſible degree of poſitive electricity; ſhewing that the tin plate had been electrified negatively by the hand *.

Exp. V. In order to examine the electricity of the atmoſphere with the multiplier, I at firſt uſed to fix a long pointed wire into the ſocket of the plate A, and then expoſed it to the open air. But I have lately uſed a much better method of

* I have repeated this experiment ſeveral times with ſucceſs; yet ſometimes it has failed, perhaps owing to the various ſtate of the ſkin of the hand, or of the ſurrounding air.

accompliſhing

accomplifhing that object. I expofe, out of the window, an infulated ftick of about five feet in length, and covered with tin foil; and whilft I hold this apparatus by the extremity of its infulating handle, I touch with the other hand, for about two or three feconds, the lower part of the ftick. By this means the ftick, being free from points, acquires an electricity contrary to that of the furrounding air. I then bring it within the room, and communicate that electricity to the plate A of the multiplier, &c. But the electricity fo acquired by the infulated ftick, is generally fufficiently ftrong to affect an electrometer without the ufe of the multiplier. To examine the electricity of the rain, fnow, hail, &c. the fame apparatus muft be expofed out of a window, but the ftick muft not be touched, for in this cafe it acquires the fame fort of electricity as that of the rain, fnow, &c. and not the contrary fort, as when expofed to the air.

## EXPERIMENTS ON METALLIC SUBSTANCES.

AFTER many fruitleſs attempts, and after having ſent to the preſs the preceding part of this volume, I at laſt hit upon a method of producing electricity by the action of metallic ſubſtances upon one another, and apparently without the interference of electric bodies. I ſay apparently ſo, becauſe the air ſeems to be in a great meaſure concerned in thoſe experiments, and perhaps the whole effect may be produced by that ſurrounding medium. But, though the irregular, contradictory, and unaccountable effects obſerved in theſe experiments do not as yet furniſh any ſatisfactory theory, and though much is to be attributed to the circumambient air, yet the metallic ſubſtances themſelves ſeem to be endowed

dowed with properties peculiar to each of them, and it is principally in consequence of those properties that the produced electricity is sometimes positive, at other times negative, and various in its intensity.

The discovery of those properties of metallic bodies opens a new field of useful investigation, and renders more manifest the general or extensive influence of a fluid wonderful in its nature and action. But how far they will enable us to explain the phenomena of Animal Electricity, and of other operations of nature, are considerations which will be noticed after the recital of the experiments. — In this account I shall endeavour to select and methodise the experiments, in the best manner that the irregularity of their results seems to admit of.

Exp. I. A piece of zinc, which weighed little more than half an ounce, was dropped ten times successively upon an insulated tin plate. This plate was then brought in contact with the plate A of the multiplier: the lever was worked, and after ten additions

tions of electricity, the plate C communicated to the electrometer a sufficiently sensible quantity of positive electricity, which shews that the tin plate had been electrified negatively by the contact of the zinc. This experiment was repeated four times within the space of half an hour, and was constantly attended with the like effect; but on the following day the effect was found to be less conspicuous, for three times twenty additions just enabled the plate C to communicate a sensible degree of positive electricity to the electrometer. In short, the different states of the atmosphere seem to be much concerned in the result of this experiment, and yet the whole effect cannot be attributed to it; but of this farther on. Before however I proceed to the narration of other experiments, it will be necessary to dwell a little longer on the abovementioned operation, not only to render it more intelligible, but likewise to avoid repetitions.

The tin plate used in the preceding as well as in many of the subsequent experiments, measures eight inches in diameter;

and is faftened to a fmall piece of wood about three inches in length. Two glafs fticks covered with fealing-wax are cemented into this piece of wood, and their other extremities are cemented into a larger piece of wood, which forms the ftand or bafis of the inftrument. The operation is as follows: I hold this apparatus by the laft-mentioned piece of wood in my left hand, and keeping the plate in an horizontal fituation, let the piece of zinc or other metallic body, fall upon it from my right hand, which I hold a few inches above the plate; then by inclining or fhaking the plate, the piece of metal is caufed to fall upon the table or upon a chair; from whence I take it up, and again let it fall upon the tin plate, and fo on.—During this operation, the tin plate muft not be touched by any thing elfe befides the piece of metal which is under examination. As the glafs fticks which fupport the tin plate, are never touched, there is no poffibility of inducing any other electricity upon it, befides that which is produced by the contact of the other metal; but to avoid fufpicion, whenever I am in doubt of the refult of any ex-

 periment,

periment, I always try the tin plate before I repeat that experiment, viz. apply it to the plate A of the multiplier, then work the lever about thirty times, and after this if I find that the plate C of the inſtrument communicates no electricity to the electrometer, which is generally the caſe, I conclude that the plate is fit for the experiment.

Exp. II. A piece of zinc that weighed about ſix ounces, was dropped ten times ſucceſſively upon the tin plate. The plate was then applied to the multiplier in the uſual manner, and after ten additions the electrometer was electrified poſitively as in the firſt experiment, but to a greater degree.

Exp. III. The preceding experiment was repeated with only this variation, viz. that the zinc was now heated to about 110°. —The reſult was a higher degree of electrization; for after twenty additions the plate C cauſed the pendulums of the electrometer to touch the glaſs veſſel in which

they were contained :—The electricity was positive.

I repeated the two last experiments several times, using the large piece of zinc alternately hot and cold; but the effects though not precisely alike, yet they differed only in degree.

Exp. IV. A shilling was dropped ten times upon the tin plate, which was afterwards applied to the multiplier, and after ten additions the electrometer was made to diverge with positive electricity. In this experiment the divergency was not very great, but quite sufficient to shew that the electricity was positive.

This experiment being repeated several times, was attended with various events, the electricity being sometimes greater than at others, and once or twice it gave no electricity at all.

Exp. V. The preceding experiment was repeated with a half crown; but after fifty additions

additions the electrometer was caused to diverge very little with positive electricity.

Exp. VI. The half crown was made very hot, and was then let fall as before ten times upon the tin plate, &c. but the success was not dissimilar from that of the preceding experiment.

Exp. VII. A new guinea was dropped ten times upon the tin plate, after which the tin plate was applied to the plate A of the multiplier, &c. After twenty additions the electrometer acquired a considerable degree of positive electricity.

Exp. VIII. The guinea was heated to about 130°, and was then treated exactly as in the preceding experiment. The electrometer received a small degree of positive electricity.

The two preceding experiments were repeated several times alternately, and the guinea was heated to different degrees; but from all those experiments it appeared, that the produced electricity was greater when

when the guinea was cold, and lefs when the guinea was hot. Indeed in the latter cafe it was fometimes next to nothing, fo that it could be hardly manifefted by three or four repetitions of about twenty additions.

Exp. IX. A piece of copper, being a new halfpenny, was dropped ten times upon the tin plate; this plate was then applied to the receiving plate A of the multiplier, and the lever was worked in the ufual manner. After twenty additions the electrometer was electrified pofitively by the plate C. The divergency of the pendulums was about an eighth of an inch.

Exp. X. The preceding experiment was repeated with this difference only, viz. that the piece of copper was firft heated to about 130°.—The electrometer acquired a fmaller degree of pofitive electricity.

Exp. XI. A piece of malleable platina that weighed about three ounces, was dropped ten times upon the above-mentioned tin plate, &c. After three times ten additions the

the electrometer ſhewed no divergency whatever; I therefore repeated the experiment ſeveral times, examining every part of the apparatus, and uſing every precaution that I could think of; but the reſult was, that the electrometer was either not at all, or very ſlightly, electrified; but when it was electrified I could plainly perceive that its electricity was poſitive.

Exp. XII. The platina was heated to about 130°, and in that ſtate was dropped ten times upon the tin plate, &c.—After twenty additions the electrometer diverged more than half an inch with negative electricity. This being an unexpected effect, I was naturally induced to repeat the experiment, not only under the ſame, but likewiſe under ſome variety of circumſtances. The general reſult was, that when the platina was uſed cold, viz. of the temperature of the air of the room, the electrometer acquired either no electricity at all, or a very ſmall degree of poſitive electricity; but when the experiment was performed with the platina heated to above 100°, the electrometer acquired the negative electricity;

which, upon the whole, was ſtronger when the platina was heated to a greater degree. Sometimes the electricity was ſo ſtrong, that after twice twenty additions the electrometer would ſtrike againſt the ſides of the glaſs veſſel.

Exp. XIII. A piece of lead, that weighed ſomewhat more than a pound, was dropped ten times upon the tin plate. After twenty additions, the pendulums of the electrometer diverged to about a quarter of an inch with poſitive electricity.

Exp. XIV. The preceding experiment was repeated with this variation, viz. that the lead was heated to about 130°. The electricity produced was very little, but negative; the pendulums of the electrometer diverging about one-tenth of an inch after three times twenty additions.

Exp. XV. The ſame experiment was repeated with the lead made hotter, and after twice twenty additions the electrometer diverged upwards of half an inch with negative electricity.

Exp. XVI. The ſame experiment was repeated with the lead made ſtill hotter, but the electricity obtained, though negative as before, was however much leſs than in the preceding experiment; ſo that there ſeems to be a maximum in the degree of heat required, for the production of the greateſt quantity of negative electricity.—This limit is obſervable with ſome other metallic ſubſtances as well as with lead.

Exp. XVII. A piece of iron was dropped ten times upon the uſual tin plate, which was afterwards applied to the receiving plate of the multiplier, &c. The ſame piece of iron was alſo uſed in different degrees of heat; inſtead of this ſmall piece a much larger lump of caſt iron was tried in the like manner; but the reſult of thoſe experiments was very equivocal, the electricity acquired by the electrometer, after twice or three times twenty additions, being always very weak, ſometimes negative, at other times poſitive, and often not perceptible. It ſeemed to follow no certain law.

Exp. XVIII. A piece of grain tin, that weighed about four ounces, was dropped ten times upon the tin plate, &c. After twice twenty additions the pendulums of the electrometer diverged about a quarter of an inch with positive electricity.

Exp. XIX. The same piece of tin was heated to about 130°, and was then treated exactly as in the preceding experiment.—The electricity obtained was stronger but still positive. In other subsequent trials this increase of electricity kept pace with the increase of heat; but when the tin was heated so as to be not far from a melting heat, the electricity though of the same quality, was however less in quantity than in lower temperatures.

Exp. XX. The tin being cooled down to the temperature of the air, I repeated the 18th Experiment with this variation, viz. that instead of the hand as used in the preceding trials, the tin was dropped upon the plate from a pair of iron tongs, and after it had been let fall from the tin plate upon a chair, it was from thence picked up again

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by means of the ſame iron tongs which I held in my right hand. After twice twenty additions the electrometer diverged very little but with negative electricity.

Exp. XXI. The preceding experiment was repeated with the tin made hot, and the electricity obtained was of the ſame quality but greater in quantity. The remarkable difference produced by the tin being dropped from the hand or from the iron tongs; the former electrifying the electrometer poſitively, and the latter negatively, naturally induced me to repeat theſe ſingular experiments ſeveral times, uſing alternately the iron tongs or the immediate application of the hand, but the reſults were generally alike; I ſay generally, becauſe it happened two or three times, amongſt twenty experiments and upwards, that the electricity obtained was different from what I expected. Whether thoſe unuſual effects originated from ſome overſight in the operation, or are to be attributed to ſome other circumſtance, I cannot poſſibly pretend to determine. Certain it is, that the electricity of the ſurrounding air, and perhaps

haps ſome other cauſes, frequently concur to alter the effect, or intirely to change the electricity from poſitive to negative, or from the latter to the former, which will be more evidently ſhewn by the following experiments and obſervations.

Exp. XXII. A piece of biſmuth, weighing about an ounce, was dropped in the uſual manner ten times upon the tin plate. This plate was then applied to the receiving plate of the multiplier, and after twenty additions the electrometer diverged with negative electricity.

Exp. XXIII. The preceding experiment was repeated ſeveral times over, with the piece of biſmuth made ſucceſſively hotter and hotter. The reſult was, that the quantity of electricity gradually became ſmaller and ſmaller, and at laſt it became poſitive, but its quantity was then very ſmall indeed; for after three times twenty additions, the pendulums of the electrometer diverged about one-tenth of an inch. It was however ſufficient to ſhew, that in biſmuth the electrical phenomena follow an

order different from that of the generality of metallic ſubſtances; for with ſeveral of them the heat ſeems to diminiſh the poſitive electricity, and at laſt to change it to negative, whereas with biſmuth heat changes it from negative to poſitive.

The peculiar properties of biſmuth, as ſhewn by the two preceding experiments, naturally ſuggeſted the neceſſity of examining very particularly into the concurring circumſtances, in order to diſcover whether the peculiar effects produced by the experiments on biſmuth, were really owing to the nature of that ſemimetal or to ſome accidental circumſtance. The firſt ſuſpicion fell upon the ſhape of it; for the piece, which I had uſed, was a fragment from a larger piece. It had been broke off by a ſtroke of the hammer, and was of courſe very irregular, and full of ſharp edges and points, ſome of which frequently broke off in the act of dropping the piece upon the tin plate. To put therefore this poſſible ſource of error out of the queſtion, I fuſed a quantity of biſmuth in a crucible, and by pouring part of it upon a marble ſlab,

ſlab, I obtained a ſmooth and flat piece as like as poſſible to a piece of tin, which I had already uſed in theſe experiments, and which, I knew, would electrify the electrometer poſitively when treated in the above-deſcribed manner. With thoſe two pieces I repeated the experiment a great number of times, alternately uſing the tin and the biſmuth, and took care to perform every other operation exactly in the ſame manner; but when the piece of tin was uſed, the electricity acquired by the electrometer was poſitive, and when the biſmuth was tried, the electricity was negative, excepting when either of the two metallic pieces was too much heated. Theſe experiments therefore ſeem to prove, beyond the poſſibility of doubt, that there is ſomething in the very nature of a metallic body, that diſpoſes it to part with or to attract the electric fluid more eaſily in certain circumſtances, than other ſubſtances of the ſame claſs.

One of the powers which I have hitherto found capable of altering or changing that tendency of metallic bodies, to part with

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or attract the electric fluid, is the action of heat. Had that action produced the same change in them all, an hypothesis might perhaps have been formed on the principle of rarefaction and condensation; but we find the case to be otherwise.

Exp. XXIV. I had the curiosity to try whether the application of the iron tongs to the bismuth, was attended with any peculiar phenomenon, and for this purpose I dropped the piece of bismuth from the tongs upon the tin plate, exactly as I had done with the tin in Experiment the 20th. The result was, that the electricity imparted to the electrometer was now positive, whereas when the hand was immediately applied to the bismuth, the electrometer was electrified negatively. I own that I am at a loss to account for those phenomena. It is however evident, that different metallic bodies have a different affinity, or attraction, towards the electric fluid; and that in consequence of this disposition, (which seems to be inherent in their natures, and not merely owing to external configu-

configuration) their contact with one another, or with the air, is ſufficient to diſturb the electric equilibrium.

Hitherto the metallic bodies had been dropped ſucceſſively upon the ſame ſubſtance, viz. the tin plate; and by this means I endeavoured to form a table of their reſpective properties; but the great difference obſerved in the reſults of ſimilar trials repeated at different times, and eſpecially under different ſtates of the atmoſphere, proved, firſt, that great accuracy in the determination of thoſe properties was not to be expected; and, ſecondly, that the phenomena obſerved in theſe experiments are in a great meaſure to be attributed to the ſurrounding air, or to the diſpoſition of the weather. In order to carry on this inveſtigation, my next object was to drop the metallic bodies upon ſome other inſulated metal, and for this purpoſe I faſtened a large ſilver ſpoon upon an inſulating ſtand, like that of the tin plate uſed in the foregoing experiments.

Exp. XXV.

Exp. XXV. Into this ſpoon I dropped a piece of zinc from my hand ten times repeatedly; then preſented the ſpoon to the receiving plate of the multiplier, and worked the lever in the uſual manner. After twenty additions the electrometer was electrified poſitively by the plate C; its pendulums diverging about half an inch. This experiment having been repeated five or ſix times in the ſame hour, was always attended with the like effect.

Exp. XXVI. The preceding experiment was repeated with the zinc heated to about 110°. The electricity obtained after twenty additions, was poſitive but ſtronger than before.

Exp. XXVII. The ſame experiment was tried again with the zinc heated to a higher degree; and after a ſimilar operation, the electricity obtained was ſo ſtrong that the pendulums of the electrometer diverged full one inch.

Exp. XXVIII. In order to carry this experiment ſtill farther, I cooled the ſpoon

out of the window, and made the piece of zinc ſo hot, that I could with difficulty handle it. The reſt of the experiment was performed in the ſame manner as before. After twenty additions, the pendulums of the electrometer ſtruck the ſides of the glaſs veſſel. The electricity was poſitive as before.

Exp. XXIX. The temperatures of the two metallic bodies were reverſed, viz. the ſpoon was heated, and the zinc was cooled out of the window; the other operations were performed in the uſual manner. The electricity obtained was poſitive, but ſo weak that, after twice twenty additions, the pendulums of the electrometer hardly diverged one-eighth of an inch. On the ſame day I repeated this experiment over and over again; ſometimes heating the ſpoon, and at other times heating the zinc to various degrees of heat. The event upon the whole was, that the electricity imparted to the electrometer was always poſitive. It was ſtrongeſt when the ſilver ſpoon was cold and the zinc hot; but it decreaſed gradually as they came nearer to the

the ſame temperature, and followed the ſame decreaſe beyond that limit, viz. when the ſilver ſpoon was uſed hot and the zinc cold. But it is very extraordinary, and indeed it ſurprized me a good deal to find, on repeating the ſame experiments on two ſucceeding days, that the electricity obtained was ſo very weak, that it could be juſt obſerved when the ſpoon was of the uſual temperature of the atmoſphere, and the zinc was heated a little. Every precaution I could think of, was found inſufficient to produce the ſame phenomena, as I had obſerved in the 25th, 26th, 27th, 28th, and 29th Experiments. The only difference I could remark was in the diſpoſition of the atmoſphere; on the firſt day the wind being eaſterly, and the air very clear and dry; whereas on the other days the weather was rainy, and the wind was weſterly.

Moſt of thoſe experiments have been repeated ſince in different ſtates of the weather, and with ſome variety in the operation, but no determinate law could be diſcovered; the electricity obtained being

ſometimes ſtrong, and at other times ſo weak, as to be manifeſted with difficulty. The ſtate of the atmoſphere is certainly concerned in it, the electricity being generally, though not always, ſtronger in dry and cold weather. In that ſtate of the weather I leave the ſilver ſpoon or tin plate in its uſual temperature, heat a piece of zinc or tin to about 110° or 120°, throw it from my hand into the ſpoon about a dozen of times, then apply the ſpoon to the receiving plate of the multiplier, and after twice twenty additions, I almoſt always obtain ſome poſitive electricity, which ſhews that the ſpoon is electrified negatively by the contact of the zinc or tin. When the experiment is repeated exactly in the ſame manner, but with biſmuth inſtead of zinc or tin, the electricity obtained is negative, ſhewing that the ſpoon is electrified poſitively by the contact of the biſmuth, eſpecially when the biſmuth is cold; but if it be heated to about 200° and upwards, the electrometer will be electrified poſitively; it ſeems therefore evident, that the production of different electricities is owing to the different natures of the metallic ſubſtances;

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for whether the electricity be derived from the air, from the hand, or merely from the contact of the two metals, its different quality cannot depend upon any other circumstance than the particular nature of the metal employed, that being the only thing varied in the experiment.

Exp. XXX. One day, when these experiments succeeded remarkably well, I was desirous of observing whether by dropping the zinc upon the spoon a different number of times, viz. by dropping it once or twice, or any other number of times, the electricity thereby obtained kept increasing in the same proportion. Upon the whole, I found that when the zinc was dropped about thirty times, the electricity produced seemed to be at the maximum; but from ten or fifteen to thirty, the difference is hardly perceptible. When the zinc is dropped four or six times, the electricity is certainly not so considerable as when it is dropped more than ten times.

Exp. XXXI. In order to discover the source of the electricity produced in these

experiments, I tried the experiment in a great variety of ways, viz. inſtead of the hand, I dropped the zinc from a tin plate held with one hand, into the ſpoon; and from the latter back upon the former. I performed this operation when both the tin plate and the ſilver ſpoon were inſulated; I likewiſe tied a ſilk thread to the zinc, and holding the other extremity of the thread in one hand, ſtruck the zinc repeatedly againſt the ſpoon; but in thoſe caſes very ſeldom any electricity was manifeſted, except when the weather and every other circumſtance was very favourable, and then the electricity could with difficulty be manifeſted. However, when the tin plate is held in the hand, and the zinc is thrown from it into the inſulated ſpoon, ſome electricity is more frequently produced than in the other two caſes.

After a careful review of the foregoing experiments, I ſtill doubt whether the phenomena of Animal Electricity may be attributed to the electricity generated by the contact of metallic bodies. Had the action of the ſame metals upon prepared ani-

mal bodies produced effects as fluctuating as thoſe of the preceding experiments, we might without heſitation have admitted the propoſition; but when we find that in different ſtates of the atmoſphere, the action of metallic bodies produces the contractions of prepared animal limbs, with hardly any obſervable difference; and when we conſider that zinc and biſmuth, which though in the above-mentioned experiments do more generally produce different ſtates of electricity, yet they are not more powerful exciters of Animal Electricity, than zinc and ſilver, or zinc and gold; we can then hardly conclude with ſaying, that the effects obſerved in Animal Electricity are to be intirely attributed to the electricity produced by the contact of the metallic bodies.

It may indeed be ſaid, that if the mutual contact of metals amongſt themſelves produces a ſenſible quantity of electricity in moſt caſes, it is likely that they always produce ſome electricity, which may be too little to be rendered manifeſt by the multiplier and an electrometer, but may yet be

ſufficient to produce the contraction of animal muſcles. I muſt however obſerve, that the quantity of electricity, which is neceſſary to contract a prepared animal limb, does not ſeem to be ſo ſmall as that which is produced when two ſmall pieces of metal come to touch each other. When a Leyden phial is uſed in experiments on prepared animals, the quantity of electricity employed cannot be properly eſtimated by means of an electrometer. Thus a Leyden phial, after having been charged and diſcharged, may contain a reſiduum, which is abſolutely incapable of affecting an electrometer; yet when condenſed upon a ſmall piece of metal, may be ſufficient not only to affect an electrometer, but to produce even a ſpark. In order therefore to compare, in a leſs equivocal manner, the quantity of electricity which can affect an electrometer, with that which is ſufficient to contract the prepared animals, I electrified an electrometer until its pendulums diverged to about a twentieth of an inch, then touched the electrometer with an inſulated piece of metal, the ſurface of which was about 200 times greater than that of the conducting

conducting parts of the electrometer, and withdrew it immediately after. By this means it is evident, that the electrometer was left with about the 200th part of that electricity, which had been at first communicated to it. On touching the prepared legs of a frog with this electrometer, possessed of that small quantity of electricity, no contraction ensued. I have repeated this experiment many times, and have found that the motions are very seldom excited by the passage of so small a quantity of electricity; yet I am sure, that in the experiments of touching one metal with another, the electricity produced is very frequently much less than that which was possessed by the electrometer just mentioned. Nevertheless, if a prepared animal be touched with those metals, the convulsions are immediately excited. Are we then not authorized to say, that either the quantity of electricity which is produced in the act of touching animals with metallic substances, is greater than that which is produced by the contact of one metal with another metal; or that the contractions excited in animal bodies by the application of metals, must be attributed

to

to ſome other property of metallic ſubſtances, independent of electricity? — It ſeems, however, proper to ſuſpend for the preſent any haſty concluſion, which may miſlead the inquirers into this new ſubject, and to wait for farther diſcoveries, and more deciſive experiments. I ſhall therefore conclude this ſection, with a ſhort enumeration of the particulars which the preceding experiments ſeem to eſtabliſh.

1. The contact of one metallic ſubſtance with another generally produces electricity.

2. The quantity and quality of the electricity ſo produced, is various according to many circumſtances which ſeem to concur in the production of it, or in great meaſure to influence it.

3. Thoſe circumſtances are, the various nature of the metallic ſubſtances, their various degrees of heat, the ſtate of the atmoſphere, and the other body concerned in the experiment, viz. the hand of the operator, &c. Each of thoſe cauſes has a ſhare

in

in the reſult of the experiment; for the variations of any one of them, when every thing elſe remains unaltered, produce different effects. Thus in different ſtates of the atmoſphere, the very ſame metallic ſubſtances treated exactly in the ſame manner, produce a greater or leſs quantity of electricity. Thus alſo, by only heating or cooling the metals, the electricity may be varied in quantity and even in quality.

I am inclined to ſuſpect, that different bodies have different capacities for holding the electric fluid, as they have for holding the elementary heat; if however the experiments relative to this ſubject be carefully tried, under all the variety of circumſtances which the combination of the above-mentioned cauſes is capable of producing, I do not doubt but that all the phenomena obſerved in the preceding pages may hereafter be reconciled to one, or to a few, ſimple laws, which will at the ſame time aſſiſt the farther inveſtigation of the ſcience of electricity.

## DESCRIPTION of the METHODS

### Of producing diverſe curious CONFIGURATIONS by MEANS of

## ELECTRICITY.

IN the ſeventh chapter of the fourth part of this treatiſe, mention is made of ſome configurations deſcribed by Profeſſor LICHTENBERG, of Gottingen, who firſt obſerved them upon a large electrophorus; and in the ſame chapter ſeveral experiments are related, which were made in explanation of thoſe phenomena. Since the original diſcovery of thoſe configurations by Profeſſor LICHTENBERG, diverſe methods have been contrived of producing them, and of fixing them upon paper, ſilk, reſinous ſubſtances, &c. Their various ſhapes and ramifications afford a good deal of information to the ſpeculative electrician, by ſhewing in many caſes the direction, diviſion, and qua-

§ lity

lity of the electricity in a convincing and permanent manner. But independent of this scientific instruction, some of those impressions are very beautiful ornaments, and as such, it is not unlikely, that they may hereafter be introduced in various manufactures.

The principal method of producing those impressions is, in general, to electrify a perfect or imperfect electric, and then to throw certain powders upon it, which will dispose their particles into various forms. Those powders are projected different ways, which, to avoid repetition, will be here described previous to the particular methods of forming those impressions. The powders may be sifted over the electrified body from a common sieve; they may be tied up in linen rags and shook out of them; they may be projected by means of a brush *, and by means of a pair of bellows; but a more

* This may be done by taking a little of the powder between a finger and thumb, and drawing it over the brush, or by rubbing upon the brush a lump of some substance that is easily reduced into powder, such as chalk, whiting, &c.

commodious

commodious method is the following: To the neck of a ſmall bottle of elaſtic gum, which is more commonly called *India rubber*, tie a tube of glaſs, wood, or metal; put the powders which you want to project, into the bottle, and then tie a double piece of flannel over the aperture of the tube. If this bottle ſo prepared be held in the hand, and be ſqueezed, by alternately opening and ſhutting the hand, the powders will be projected in a fine diffuſed manner.

As for the nature of the powders, almoſt every ſubſtance that can be pulverized ſufficiently fine, will produce ſome configurations when projected upon an electrified ſubſtance. Thus chalk, ſulphur, cinnabar, roſin, dragon's blood, gum arabic, evaporated decoctions of colouring woods, and many others, may be employed for this purpoſe either ſingle or mixed.

Exp. I. Take a pane of glaſs clean and dry, hold it ſuſpended by one corner, or lay it flat upon a table, and draw over the ſurface of it the knob of a Leyden phial, moderately

derately charged with positive electricity. Then lift up the glass, if laid upon a table, and, holding it suspended, project upon it, by means of the elastic gum bottle, a mixed powder consisting of dragon's blood and gum arabic in equal parts. You will find that the two powders will be separated upon the glass; the red powder of dragon's blood falling on certain places, and the whitish powder of gum arabic falling on certain other places, so as all together to form an oblong radiated track, consisting of two colours, intermixed in a thousand odd ways *.

If instead of drawing the knob of the jar over the surface of the glass, you only touch the surface here and there with it, and then throw the mixed powder upon it as before; separate star-like figures will be formed about those points. The stars,

* Having shewn in the 7th chap. of the 4th part, that in the act of projecting those powders some of them acquire the positive, and others the negative electricity, I need not repeat, in this place, the reasons why the dragon's blood and gum arabic take different places in this or in the following experiments.

however,

however, are better defined when a ſingle powder is projected: their rays or ramifications are ſometimes few and ſtrong, at other times many and ſlight; and frequently they don't go quite round the points on which the knob of the jar has been touched *. At A and B of fig. 6th, in the annexed plate, two of thoſe ſparks are delineated, in one of which the ramifications go quite round, but in the other they do not.

Exp. II. Repeat the preceding experiment with only this variation, viz. that now the Leyden phial be charged negatively, and the appearance of the configuration will be vaſtly different from that occaſioned by poſitive electricity. In this very few rays or branches will be obſerved; the powders moſtly diſpoſing themſelves in roundiſh ſpots, and generally a central ſpot of one powder will be ſurrounded by another powder of another colour.

* Theſe various effects depend principally on the greater or leſſer quantity of electricity contained in the Leyden phial.

Inſtead

Inſtead of dragon's-blood and gum arabic, powders of other colours may be projected upon the pane of glaſs, ſuch as powder of Pruſſian blue, ſulphur, vermilion, roſin, &c.; and thus the colours of the configurations may be varied.

Thoſe powders adhere to the glaſs rather ſlightly, viz. ſo as not to bear being touched; yet if a piece of paper be laid on the painted ſide of the glaſs without rubbing it, and the edge of it be glued all round the edge of the glaſs, the figures may be preſerved without injury. But a better method is, to lay another pane of glaſs of the ſame ſize upon the glaſs with the configurations, and to faſten them round with ſealing-wax, or a ſlip of paper paſted over both their edges.

If powders of ſuch colours as are uſed for enamel-painting, be projected upon glaſs or porcelain, and theſe ſubſtances be afterwards expoſed to a proper degree of heat in an enameller's furnace, the configurations will thereby be rendered indelible.

Exp. III. Take a piece of common writing paper, hold it very near the fire, ſo as to render it quite dry and very hot; lay it flat upon a dry marble ſlab or a very dry table, and in that ſituation draw over it the knob of a charged Leyden phial, then lift up the piece of paper by one corner, and holding it ſuſpended, project upon it the mixed powder of dragon's-blood and gum arabic by means of the elaſtic gum bottle. The configurations in this caſe are very beautiful, and may be made in various ſhapes, as of letters, ſtars, ſtripes, &c. by moving the knob of the Leyden phial in the deſired direction; but they are of one colour, viz. red; for the gum arabic being nearly of the colour of the paper, cannot be diſtinguiſhed upon it.

If the paper thus painted be held very near the fire for a few ſeconds of time, the powder of dragon's-blood, being a reſinous ſubſtance, will be melted, and faſtened to the paper; after which the powder of gum arabic may be wiped off with a handkerchief.

Powders

Powders of other colours may be projected upon the paper after the ſame manner; but unleſs they are of a reſinous nature, ſo as to be eaſily melted by heat, it is very difficult to faſten them to the paper.

A little experience will ſoon enable the operator to make thoſe impreſſions in a neat and definite manner; nevertheleſs it will be proper, before we deſcribe the other experiments, to mention ſome uſeful precautions. The charge of the Leyden phial for theſe experiments muſt be neither too high nor too low; for in the former caſe, the figure obtained will be too confuſed and irregular, and in the latter it will be too faint. In order to form a neat and well-determined figure, and to leave the reſt of the paper clean, the powders muſt not be projected perpendicularly to the paper, but the ſtream muſt be thrown in a direction parallel to the ſurface of the paper. It is alſo neceſſary to perform theſe experiments in as quick a manner as poſſible, for if the paper be ſuffered to cool too much, or the communicated

electricity to diffipate, the defired effect will not be obtained.

Exp. IV. Inftead of the paper, the impreffions may be made upon marble, by drawing the knob of the charged jar upon it, in the fame manner as it is done upon paper. For this purpofe, the marble muft be made dry and hot. After the fame manner, the configurations may be made upon all forts of electric or femi-electric fubftances, and they may be preferved by covering them with a pane of glafs, by partially melting either the refinous plate or the powders, if they are of a proper nature; or, laftly, by tranfpofing them from the electric plate upon a piece of paper. But this laft method is attended with a good deal of difficulty. I fhall, however, (for the fake of thofe perfons who are not unwilling to undergo the trouble attending it) fubjoin Mr. BENNET's method of performing it, which is very minutely defcribed in his work on Electricity.

"To make red figures, take a pound of " rafped Brazil wood, put it into a kettle

" with as much water as will cover it, or " rather more; alſo put in about an ounce " of gum arabic and a lump of allum about " as big as a large nut, let it boil about " two hours, or till the water is ſtrongly " coloured; ſtrain off the extract into a " broad diſh, and ſet it in an iron oven, " where it is to remain till all the water be " evaporated, which with me was effected " in about twelve hours; but this depends " on the heat of the oven, which ſhould " not be ſo hot as to endanger its burning. " Sometimes I have boiled the ſtrained ex- " tract till it was conſiderably inſpiſſated " before it was placed in the oven, that it " might be ſooner dry.

" When it is quite dry but not burnt, " ſcrape it out of the diſh, and grind it in a " mortar till it be finely pulverized. In " doing this, it is proper to cover the mor- " tar with a cloth, having a hole through " to prevent the powder from flying away " and offending the noſe, and alſo to do it " out of doors if the weather be dry and " calm, that the air may carry away the " powder neceſſarily eſcaping, and which

"otherwiſe is very diſagreeable. When "ground fine, let it be ſifted through muſ-"lin or a fine hair-ſieve, returning the "coarſer part into the mortar to be ground "again. When the grinding and ſifting "are finiſhed, the powder is ready for uſe. "The reſinous plate I have moſtly uſed "was compoſed of five pounds of roſin, "half a pound of bees-wax, and two "ounces of lamp-black, melted together, "and poured upon a board ſixteen inches "ſquare, with ribs upon the edges at leaſt "half an inch high, to confine the compo-"ſition whilſt fluid; thus the reſinous plate "was half an inch thick, which is better "than a thinner plate, the figures being "more diſtinct. After the compoſition is "cold, it will be found covered with ſmall "bliſters, which may be taken out by hold-"ing the plate before the fire, till the ſur-"face be melted, then let it cool again, and "upon holding it a ſecond time to the fire, "more bliſters will appear; but by thus "repeatedly heating and cooling the ſur-"face, it will at laſt become perfectly "ſmooth. Some plates were made ſmaller, "and the reſinous compoſition confined to

" the form of an ellipsis, a circle or escut-
" cheon, by a rim of tin half an inch broad,
" and fixed upon a board.

" The next thing to be done is to prepare " the paper, which is to be softened in wa- " ter, either by laying the pieces upon each " other in a vessel of cold water, or first " pouring a little hot water upon the bot- " tom of a large dish, then laying upon it a " piece of paper, so that one edge of the " paper may lie over the edge of the dish, " to remain dry, that it may afterwards be " more conveniently taken up. Then pour " more hot water upon its upper surface. " Upon this place another piece in the same " manner, again pouring on more water, " and thus proceed till all the pieces are " laid in. By using hot water, the paper " will be more softened in a few minntes " than if it remains in cold water a whole " day.

" When the figures are to be made, the " resinous plate must lie horizontally, whilst " the electricity is communicated, if the " experiment requires any thing to be placed

" upon the plate: but it is convenient af-
" terwards to hang it up in a vertical posi-
" tion whilst the powder is projected, lest
" too much powder should fall where it is
" not required.

" A little of the powder may be taken
" between a finger and thumb, and pro-
" jected by drawing it over a brush, or,
" which is better, a quantity of powder
" may be put into the bellows and blown
" towards the plate. When the figure is
" sufficiently covered with powder, let the
" plate be again laid horizontally upon a
" table, then take one of the softened pa-
" pers out of the water by its dry edge,
" and lay it carefully between the leaves of
" a book, pressing the book together, and
" let it lie in this situation about half a
" minute. Then remove the paper to a
" dry place in the book, and press it again
" about the same time, which will generally
" be sufficient to take off the superfluous
" moisture. Then take up the paper by
" the two corners of its dry edge, and place
" the wet edge a little beyond the figure on
" the resinous plate, lowering the rest of
" the

"the piece gradually till it covers the figure "without ſliding, then lay over it a piece "of clean dry paper, and preſs it gently, "let it remain a ſhort time, and then rub "it cloſer to the plate with a cloth, or, "which is better, preſs it down by means "of a wooden roller covered with cloth, "taking care that the paper be not moved "from its firſt poſition. When the paper "is ſufficiently preſſed, let it be taken up "by its dry edge, and laid upon the ſurface "of a veſſel of water with the printed ſide "downwards, by this means the ſuper- "fluous powder will ſink in the water, and "the figure will not be ſo liable afterwards "to ſpread in the paper. After the paper "has remained on the water during a few "minutes, take it up and place it between "the leaves of a book, removing it fre- "quently to a dry place. If it be deſired "that the paper ſhould be ſpeedily dry, let "the book-leaves in which it is to be "placed be previouſly warmed, and by re- "moving it to ſeveral places it will be dry "much ſooner than by holding it near a "fire, and without drawing the paper "crooked. By the above proceſs, it is ob-

"vious,

" vious, that leather, callico, or linen, as " well as paper, may be printed with theſe " figures, and the effects of the diffuſion " of electricity upon a reſinous plate be " exhibited to thoſe who have not leiſure " or inclination to perform the experi- " ments."

Exp. V. A very regular figure may be formed on the ſurface of a reſinous plate by the following means: Lay the reſinous plate upon a table; inſulate one or more pointed wires over the plate, with their points directed towards its ſurface, and at about the diſtance of an inch and a half or two inches. Then by touching thoſe wires alternately with a poſitive and a negative Leyden phial, throw the alternate ſparks upon the reſinous ſurface; and afterwards, by projecting different coloured powders upon the plate, you will obtain very regular figures, conſiſting of concentric zones of different colours, which aſſume the form of circles, ellipſes, or other curves, according as one or more pointed wires are uſed, and according as thoſe wires are placed nearer or farther from each other.

Exp.

Exp. VI. Cut a figure of any ſort out of the middle of a card, as for inſtance a profile, a flower, &c. Place a piece of white ſilk (white ſattin anſwers very well) upon a table; lay the card upon it, and a gold leaf over the card; in which ſituation it is evident that the gold leaf will touch the ſilk only within the limits of the figure that has been cut out of the card. This done, lay another card over the gold leaf, and put a book, or ſomething elſe heavy, upon it to keep it down. Care, however, muſt be taken to leave two projections of the gold leaf out of the card at oppoſite ends. Laſtly, if you ſend the charge of a battery through the gold leaf, by connecting one of its projections with the inſide, and the other with the outſide of the battery, the gold will be melted and forced into the ſubſtance of the ſilk, ſo as to ſtain it with a purple ſpot of the ſhape and ſize of the figure cut in the card. The battery for this experiment muſt have force ſufficient to melt the gold leaf completely. I need hardly obſerve, that by changing the metallic leaf, as well as the figure in the card,

card, many beautiful ornaments may, by this means, be marked upon ſilk.

Exp. VII. Hold a piece of writing paper near the fire to render it dry and warm: then lay it upon a table, and rub it with a dry hand, which operation will electrify the paper. Now light a piece of ſealing-wax with a candle, and after having ſuffered it to burn for about five or ſix ſeconds of time, lift up the excited paper from the table, and hold it up by one corner; blow out the flame of the ſealing-wax, and preſent the melted end of it to the paper at the diſtance of about an inch, moving it in various directions very quickly. In doing this, the electricity of the paper will attract the ſealing-wax into the ſhape of exceedingly fine filaments, which may afterwards be melted and faſtened to the paper, by holding the paper very near the fire for a ſhort time. A ſmall piece of ſealing-wax ſtuck upon a wire or a pin, anſwers better than a common ſtick of ſealing-wax. The impreſſions made in this manner, are in general not ſo beautiful as thoſe deſcribed in the preceding pages; yet this experiment

ment is attended with a conſiderable advantage, which is, that it does not require the electrical machine, or other apparatus, and may of courſe be performed in any place.

OF THE

## EFFECTS produced by ELECTRICITY

On permanently ELASTIC FLUIDS, and on WATER.

IN the preceding volumes of this treatiſe, notice has been taken of ſome effects produced by Electricity on certain permanently elaſtic fluids, and likewiſe upon water; but ſince the third edition of my treatiſe, ſome capital diſcoveries have been made relative to this ſubject, which, beſides their being remarkably curious in themſelves, are likely to unfold ſome general operations in the grand laboratory of Nature.

With reſpect to the permanently elaſtic fluids, it has been diſcovered, that by the action of Electricity a mixture of dephlogiſticated and common air, or of dephlogiſticated

ticated and phlogiſticated air, is converted into nitrous acid; and with reſpect to water it has been found, that by means of electric ſhocks, that fluid is converted into an inflammable and permanently elaſtic fluid, reſembling a mixture of inflammable and common air, which by inflammation is again converted into water. The firſt of thoſe diſcoveries is due to the genius of Henry Cavendiſh, Eſq; F. R. S. and an accurate deſcription of his experiments, &c. was publiſhed in the 75th and 78th volumes of the Phil. Tranſactions, from which the following account has been extracted moſtly in his own words.

The quantity of air through which the electric ſparks were required to be paſſed, was confined in the angular part of a glaſs tube, bent to an angle, and inverted with its two open extremities in two glaſſes nearly full of quickſilver; the remaining cavity of the glaſs tube being likewiſe filled with quickſilver *. In order to force the electric

* The bore of the tube moſtly uſed for thoſe experiments, was about one-tenth of an inch in diameter; and the

tric ſpark through the tube, a metallic ball was inſulated at a ſmall diſtance of the prime conductor of an electrical machine, from which it received the ſparks; and a communication was made between the ball and the quickſilver in one of the glaſſes, while the quickſilver of the other glaſs communicated with the ground. In theſe experiments the electrization muſt be continued for a conſiderable time. If the electrical machine works well, its prime conductor giving between two and three hundred ſparks in a minute to the ball, and if the machine be worked for about half an hour a day, the experiment will hardly be completed in a fortnight or three weeks time.

" When the electric ſpark was made to " paſs through common air, included be-

the length of the column of air occupying the upper, and middle part of the tube, was in general from 1½ to ¾ of an inch. The tube was firſt entirely filled with quickſilver, and was inverted with its extremities into the quickſilver contained in the two glaſſes; then the proper quantity of air was introduced in it by means of a well-contrived apparatus, which is minutely deſcribed in the 75th vol. of the Phil. Tranſ. p. 372, and following.

" tween

" tween short columns of a solution of " litmus, the solution acquired a red co- " lour, and the air was diminished con- " formably to what was observed by Dr. " Priestley.

" When lime-water was used instead of " the solution of litmus, and the spark was " continued till the air could be no further " diminished, not the least cloud could be " perceived in the lime-water; but the air " was reduced to two-thirds of its original " bulk, which is a greater diminution than " it could have suffered by mere phlogisti- " cation, as that is very little more than one- " fifth of the whole.

" The experiment was next repeated " with some impure dephlogisticated air; " the air was very much diminished, but " without the least cloud being produced " in the lime-water. Neither was any " cloud produced when fixed air was let up " to it; but on the further addition of a " little caustic volatile alkali, a brown sedi- " ment was immediately perceived.

"Hence we may conclude, that the "lime-water was ſaturated by ſome acid "formed during the operation; as in this "caſe it is evident, that no earth could be "precipitated by the fixed air alone, but "that cauſtic volatile alkali, on being add-"ed, would abſorb the fixed air, and thus "becoming mild, would immediately pre-"cipitate the earth; whereas, if the earth "in the lime-water had not been ſaturated "with an acid, it would have been precipi-"tated by the fixed air. As to the brown "colour of the ſediment, it moſt likely pro-"ceeded from ſome of the quickſilver hav-"ing been diſſolved."

Mr. Cavendiſh made ſeveral experiments with a view to determine what degree of purity the air ſhould be of in order to be diminiſhed moſt readily, and to the greateſt degree; and found, that good dephlogiſticated air was diminiſhed very little; perfectly phlogiſticated air was not ſenſibly diminiſhed: but a mixture of dephlogiſticated and phlogiſticated air in the proportion of five parts of the former to three parts of the

the latter, was made almoſt entirely to diſappear.

"Having made, *ſays he*, theſe previous "trials, I introduced into the tube a little "ſoap-lees, and then let up ſome dephlo-"giſticated and common air, mixed in the "above-mentioned proportions, which riſ-"ing to the top of the tube, divided the "ſoap-lees into its two legs. As faſt as "the air was diminiſhed by the electric "ſpark, I continued adding more of the "ſame kind, till no further diminution took "place: after which a little pure dephlo-"giſticated air, and after that a little com-"mon air were added, in order to ſee whether "the ceſſation of diminution was not ow-"ing to ſome imperfection in the propor-"tion of the two kinds of air to each other, "but without effect *. The ſoap-lees "being

* "From what follows it appears, that the reaſon why "the air ceaſed to diminiſh was, that as the ſoap-lees "were then become neutralized, no alkali remained to "abſorb the acid formed by the operation, and in conſe-"quence ſcarce any air was turned into acid. The "ſpark, however, was not continued long enough after "the apparent ceſſation of diminution, to determine with "certainty,

" being then poured out of the tube, and " ſeparated from the quickſilver, ſeemed to " be perfectly neutralized, as they did not " at all diſcolour paper tinged with the " juice of blue flowers. Being evaporated " to dryneſs, they left a ſmall quantity of " ſalt, which was evidently nitre, as appear- " ed by the manner in which paper, im- " pregnated with a ſolution of it, burned.

" For more ſatisfaction, I tried this ex- " periment over again on a larger ſcale. " About five times the former quantity of " ſoap-lees were now let up into a tube of " a larger bore; and a mixture of dephlo- " giſticated and common air, in the ſame " proportions as before, being introduced, " the ſpark was continued till no more air " could be made to diſappear. The liquor, " when poured out of the tube, ſmelled " evidently of phlogiſticated nitrous acid, " and being evaporated to dryneſs, yield-

" certainty, whether it was only that the diminution went " on remarkably ſlower than before, or that it was al- " moſt come to a ſtand, and could not have been carried " much further, though I had perſiſted in paſſing the " ſpark."

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"ed 1 $\frac{4}{10}$ gr. of ſalt, which is pretty exactly
"equal in weight to the nitre which that
"quantity of ſoap-lees would have afforded,
"if ſaturated with nitrous acid. This ſalt
"was found, by the manner in which pa-
"per dipped into a ſolution of it burned, to
"be true nitre. It appeared by the teſt of
"*terra ponderoſa ſolita*, to contain not more
"vitriolic acid than the ſoap-lees them-
"ſelves contained, which was exceſſively
"little, and there is no reaſon to think that
"any other acid entered into it, except the
"nitrous."

The dephlogiſticated air uſed in the foregoing experiments was not extracted from nitre, but from the black powder formed by the agitation of quickſilver mixed with lead, or from turbith mineral. "In the
"firſt experiment, the quantity of ſoap-
"lees uſed was 35 meaſures, each of which
"was equal in bulk to one grain of quick-
"ſilver; and that of the air abſorbed was
"416 ſuch meaſures of phlogiſticated, and
"914 of dephlogiſticated. In the ſecond
"experiment, 178 meaſures of ſoap-lees
"were uſed, and they abſorbed 1920 of

" phlogiſticated air, and 4860 of dephlo-
" giſticated. It muſt be obſerved, how-
" ever, that in both experiments ſome air
" remained in the tube uncondenſed, whoſe
" degree of purity I had no way of trying;
" ſo that the proportion of each ſpecies of
" air abſorbed is not known with much ex-
" actneſs."

Some time after the publication of the above-mentioned diſcovery, Mr. Cavendiſh, in order to remove ſome difficulties which had prevented the ſucceſs of other perſons, who had attempted to repeat his experiments, and likewiſe to confirm his former aſſertions, cauſed his principal experiments to be repeated by a perſon verſed in the conduct of nice operations; but the reſults, though attended with a little variation, which was evidently owing to the trifling difference of certain circumſtances in the manner of performing the experiment, confirmed the great diſcovery, viz. that by the action of electric ſparks, a mixture of dephlogiſticated and phlogiſticated air is converted into nitrous acid, capable of

of forming nitre when combined with an alkali.

In the firſt volume of this treatiſe, the principal mechanical effects produced upon water by the electric ſparks have been ſufficiently noticed; but ſome remarkable experiments were made not long ago, in which a decompoſition of water was effected by the mere action of electric ſhocks, which converted it into an inflammable and permanently elaſtic fluid, and this elaſtic fluid being afterwards inflamed by the ſame electric ſpark, was thereby again converted into water. In order to give my readers a juſt account of thoſe valuable experiments, I ſhall ſubjoin the tranſlation of a letter of the authors themſelves, in which their experiments and obſervations are methodically and clearly deſcribed.

*LETTER of Meſſrs.* PACTS, VAN TROOSTWYK *and* DEIMAN, *to Mr.* DE LA METHERIE, *on a new Method of converting Water into dephlogiſticated and inflammable Air.*

" SIR,

" We requeſt of you to inſert in your journal, the experiments and obſervations which we have the honour of tranſmitting to you, upon a ſubject the moſt famous in chemiſtry and natural philoſophy.

" Howſoever ſatisfactory thoſe experiments may appear, upon which Mr. LAVOISIER, and moſt of the French chymiſts have eſtabliſhed their theory concerning water, it muſt nevertheleſs be acknowledged that they are not abſolutely concluſive.

" The partizans of each of the two oppoſite theories at preſent agree in the following particulars: 1ſt. That by the combuſtion of inflammable and dephlogiſticated air ſome acid as well as water is obtained:

 2dly,

2dly, That this acid was not accidentally contained in the airs employed, but that it is really formed in the act of inflammation. The same thing however cannot be said with respect to the water; for this may be supposed to have previously existed in the two airs, which may be principally proved by observing, that the dryer the airs are made before the combustion, the smaller is the quantity of water obtained after it. Upon the whole, it seems, that the adversaries of the new theory have as much right to call the water an accidental substance, as its defenders have for considering in that light the acid which is found after the combustion.

"The decomposition of water, provided it could be clearly proved, might decide this question without leaving room for the least difficulty. Water has hitherto been decomposed only by the aid of iron, a substance which, by the action of heat alone, will produce a sort of air, which is looked upon as an elementary component of water. It may therefore be supposed, that in this experiment the water serves only to disengage

gage the air more eaſily, and in greater quantity from the metal, which is naturally diſpoſed to yield it. Even this theory of the decompoſition of water is entirely founded upon an hypotheſis, which is not generally admitted, viz. that the calcination of metals conſiſts in nothing more than their combination with the baſe of dephlogiſticated air. The calcination itſelf in this experiment ſeems not to have been ſatisfactorily proved, and many philoſophers entertain doubts about it.

" Notwithſtanding what we have ſaid, to ſhew that the new theory of the French chymiſts concerning water, has not been hitherto rigorouſly proved, and though we ourſelves entertained the contrary opinion; we are, however, far from meaning to defend the old ſyſtem. On the contrary, we believe that we may contribute conſiderably towards the eſtabliſhment of the new theory; ſince we have been fortunate in the diſcovery of a method of decompoſing water, and of converting it into inflammable and dephlogiſticated air, in a manner which ſeems to exclude the poſſibility of

thoſe airs being derived from any other ſubſtance.

"Being employed (together with Mr. Cuthbertſon, who has aſſiſted us conſiderably in theſe experiments, and whom we are very willing ſhould ſhare with us the merit of the preſent diſcovery) in an inveſtigation of the effects of the electric ſhocks on different ſubſtances, we had the curioſity of obſerving its effects on water alſo. For this purpoſe we filled a tube of $\frac{1}{8}$ of an inch in diameter and one foot in length, with diſtilled water. One extremity of the tube was hermetically ſealed, and a gold wire was cloſed in it, which projected an inch and a half within the tube. The other extremity of the tube was immerſed in a ſmall glaſs veſſel full of diſtilled water, and another wire paſſed through this aperture, and went up into the tube ſo as to be $\frac{5}{8}$ of an inch diſtant from the firſt-mentioned wire. In order to tranſmit the electric ſhock from one of thoſe wires to the other, and of courſe to let it paſs through the water contained in the tube between the extremities of the two wires, we placed the cloſed

closed end of the tube against a copper-ball, that stood insulated at some distance of the prime conductor of our machine, and a communication was made from the extremity of the wire which stood in the vessel full of water, to the outside of a Leyden phial, the coated surface of which was equal to one square foot, and whose knob communicated with the prime conductor*.

"Trying with this apparatus the effects of the electric shock upon the water, the copper-ball standing at a very small distance from the prime conductor, we could not perceive any production of air. Having increased this distance, and consequently the strength of the shock, we could see at each explosion a spark on the extremity of each wire, and at the same time a great number of very small bubbles of air appear-

* The electrical machine which we have used for these experiments, consists of two plates, the diameter of each of which is 31 inches, and its construction is like that of TEYLER. (See p. 273. vol. II. of this treatise.) Its power is such as that a Leyden phial, like the one mentioned above, will be charged, and spontaneously discharged 25 times in 15 revolutions of the plates of the machine.

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ed like a continuate ſtream between thoſe two extremities. The production of air was more abundant and the bubbles were larger, when the copper-ball was placed at a diſtance ſtill greater from the prime conductor, in which caſe a ray of light was ſeen to proceed into the water from the extremity of the upper wire. The air which was thus produced, aſcended to the upper part of the tube, where it formed a column, which kept increaſing in proportion as we continued to tranſmit the electric ſhocks through the water, till it reached the extremity of the upper wire, at which time the electric ſpark, which was now obliged to paſs through it in its way to the other wire, inflamed it, preciſely in the ſame manner as it does with inflammable air, and cauſed it to diſappear, excepting a very ſmall reſiduum. Having removed that reſiduum, we again tranſmitted the electric ſhocks through the water, which produced another quantity of air, and when the column of it had reached the extremity of the upper wire, the inflammation took place, and the air diſappeared all but a ſmall reſiduum, every thing happening exactly as in the

the preceding experiment. We repeated the ſame experiment many times over, and conſtantly obſerved the ſame phenomena, excepting that the reſiduum of air remaining after the inflammation in each experiment, ſeemed to be ſmaller than in the preceding.

"We cannot therefore be any longer in doubt of having obtained inflammable air from water, viz. the ſame ſpecies of air, the baſe of which has been looked upon as a conſtituent element of water. The exploſive property of this air, which, as it is well known, cannot take place without the preſence of dephlogiſticated air, ſeems to point out the probability of the latter ſort of air being likewiſe produced at the ſame time. It is however neceſſary, in order to remove every poſſible doubt, to prove in the firſt place, that the electric matter does not in the leaſt contribute to the production of the inflammable air; and, ſecondly, that the reſpirable air, which enables it to take fire, is not to be attributed to any common air contained in the water or adhering to the ſurface of the tube. Here follow the

the experiments which we have made with a view to elucidate thoſe two points.

"The method which we thought to be the eaſieſt as well as the beſt calculated to clear the firſt difficulty, was to paſs the electric ſhocks through oil of vitriol, and through the nitrous acid, in the ſame manner as we had paſſed them through water. When thoſe acids were uſed inſtead of water, and eſpecially with the vitriolic, the production of air did alſo take place, though not ſo abundantly as from the water: but when the quantity of this air was increaſed ſo far as to go beyond the extremity of the upper wire, the inflammation did not take place; inſtead of which, the augmentation proceeded in the ſame proportion. We therefore ſuſpected that this might be the vitriolic acid air, or the nitrous acid air; and as it is known that thoſe airs are abſorbed by their reſpective acids, we left them in that ſituation for a conſiderable time; but their bulk did not ſuffer the leaſt diminution. This air therefore could not be ſuppoſed to be any thing elſe beſides the dephlogiſticated air. In order to aſcertain this point, we

we introduced a certain quantity of nitrous air into the tube, and obſerved, that as ſoon as this air came to be mixed with the air contained in the tube, a diminution of bulk enſued, like that which takes place whenever dephlogiſticated air is mixed with nitrous air.

"After having compared thoſe experiments together, we conceive we have demonſtrated, that the electric ſhock produced no other effect upon the water, beſides diſpoſing the baſe of inflammable air to aſſume its aeriform ſtate; and that in the acids it cauſes the baſe of dephlogiſticated air to aſſume that ſtate alſo. If the electric ſhock itſelf had contributed ſomething towards the formation of the inflammable air, it would not have detached from the acids the pure reſpirable air; ſince by the admixture of the inflammable principle, this air would have been contaminated: and on the contrary it would, agreeably to that principle, have produced the vitriolic acid air or the nitrous acid air. There remains, therefore, no doubt, but that the inflammable air, obtained from water, is derived from the water

ter itſelf, and that it is one of its conſtituent principles.

"The next thing to be aſcertained is, whether the dephlogiſticated air, which is ſhewn to exiſt in it by the exploſion of the inflammable air, derives its origin from the water itſelf, or from a portion of atmoſpherical air, that may either have been contained in the water, or may have adhered to the ſurface of the tube. For this purpoſe we judged it neceſſary, previous to making the experiment, to deprive both the water and the tube of whatever air they might contain, by means of the air-pump, invented by Mr. Cuthbertſon, which machine is capable of producing a greater degree of rarefaction, than any other of the kind hitherto made public.

"We then propoſed to repeat the experiment many times over, and to remove, after every inflammation, the remaining ſmall quantity of air; ſo that by repeating the production of air, and its inflammation, we might perhaps ſucceed to deprive the water of all the air, which, notwithſtanding the

purification by the air-pump, might ſtill be contained in it; and of courſe to let the entire quantity of air diſappear after the inflammation, or elſe that if ſome atmoſpherical air was concerned in it, we might at laſt prevent the inflammation entirely.

"Accordingly we repeated the experiments, and in order to prevent the poſſibility of the water abſorbing any air from the atmoſphere, we immerſed the aperture of the tube in quickſilver. And to prevent the gold wire being touched by the quickſilver, when the tube was inverted for the purpoſe of letting out the reſiduum of air, we made uſe of another tube having the ſame diameter, and the ſame length as the firſt, but bent towards its lower part in the manner ſhewn by fig. 5th. The lower wire was of gold as far only as the inferior curvature, the reſt being of platina. But this tube, and two others which we uſed after it, as well as the above-mentioned ſtraight one, were all broken after a few exploſions, which ſhewed, that the weight of the quickſilver made a great reſiſtance to the expanſion occaſioned upon the water by the electric

electric ſhock, and therefore that it was ſcarcely poſſible to uſe quickſilver in ſuch experiments. In conſequence of which, we were under the neceſſity of immerſing the open extremity of the tube again in water, but took care to uſe only water that had been deprived of air by means of the air-pump. We continued to uſe the bent tube, ſo that in caſe the atmoſphere, by being in contact with the water of the ſmall veſſel, imparted to that water and to the water of the tube, a ſmall portion of air, that air might not go farther than the upper part of the curvature of the tube. We derived another advantage from this method, which was, that by leaving a ſmall quantity of air in this part of the tube, from which no error could be derived, the water might expand itſelf with more freedom; in conſequence of which, ſtronger ſhocks could be uſed without the danger of breaking the tube, and with more advantage, ſince the ſtrong ſhocks were attended with a more copious production of air; this expedient was moreover abſolutely neceſſary, conſidering that the ſucceſs of the experiment, and the truth of the conſequences deducible therefrom,

are more complete in proportion as the operation is performed in a ſhorter time*.

"Inſtead of leaving the diſtance of ¾ of an inch between the prime conductor, and the copper-ball which received the electric ſpark, and which was in contact with the upper part of the tube full of water, we increaſed that diſtance to one inch. At the ſame time, in order to prevent the paſſage of the electric matter in the form of a continuate ray from one wire to the other (which circumſtance we had obſerved frequently to occaſion the breaking of the tube) we placed the lower wire at the diſtance of one inch and a quarter from the upper one. With this diſpoſition of the apparatus, the production of air was ſo rapid, that after 600 ſhocks, the length of the column of air was one inch and ⅗th, and had very nearly reached

* The truth of this remark was confirmed by experience, for when we tried the experiment on the following day, we were obliged to produce the air twice, and to inflame it twice, before we could obtain a reſiduum ſo ſmall as we had obtained on the preceding day, after a ſingle inflammation. This difference muſt be undoubtedly attributed to the common air, which, during the interval of time, had inſinuated itſelf into the water.

the extremity of the upper wire. But as the expanſion of the water was now very conſiderable on account of the ſtrong ſhocks, the column of air was thereby frequently interrupted; and leſt this accident ſhould happen when the column of air had reached the extremity of the wire, which would have occaſioned a partial inflammation of the air, we thought proper to diminiſh the force of the electric ſhocks. In this manner we proceeded, till the air had reached the extremity of the wire, at which time the inflammation took place, and the column of air diſappeared, excepting a ſmall bubble of $\frac{1}{16}$th of an inch in diameter. The tube was then inclined, ſo as to let the ſaid bubble come to the extremity of the wire, and by ſending an electric ſhock through it, the inflammation again took place, and half of the bubble diſappeared.

" Having expelled this ſmall reſiduum, we began to produce the air anew, which was obtained with the ſame rapidity. The operation was continued till the air reached the extremity of the wire. The reſiduum, after the inflammation, was not more than

$\frac{1}{20}$th of an inch in diameter. A ſecond inflammation, effected in the above-mentioned [illegible] the half of this reſiduum to [illegible]

"The third time the reſiduum was not more than $\frac{1}{40}$th of an inch in diameter, but it proved difficult to inflame it a ſecond time. We however ſucceeded at laſt, and there remained a bubble extremely ſmall, the diameter of which ſeemed to be about $\frac{1}{60}$th of an inch.

"The fourth time (the air having always been produced with the ſame rapidity) all the column diſappeared, excepting a ſmall bubble of about $\frac{1}{80}$th of an inch in diameter, which it was found impoſſible to inflame a ſecond time; but if we conſider the very ſmall ſize of this remaining bubble when compared to the total quantity, and likewiſe if we admit that the half of this bubble might, in all probability, have been diminiſhed by a ſecond inflammation, provided it had been practicable, we cannot but think ourſelves authorized to ſuppoſe, that the air produced is not by any means to be attributed

buted to any common air contained in the water, or at leaſt only to an exceeding ſmall quantity; which, in the firſt caſe, would have intirely diſappeared, and this muſt have alſo happened if we could have proceeded farther with the ſucceſſive inflammations.

"We beg leave to add ſome remarks on theſe experiments, and upon others that have formed the ſupport of the new theory concerning water, which theory has been confirmed by our experiments.

"We believe we have completely demonſtrated by our experiments, that water conſiſts of the baſes of inflammable, and of dephlogiſticated air. But let us collect together the principal points.

1. "We have obtained inflammable air, which is the only ſort of air capable of combuſtion; and dephlogiſticated air, which is the only ſpecies of air that can aſſiſt the combuſtion. This has been proved by the inflammation.

2. "We have obtained nothing but thoſe two ſorts of air, which is proved by the diſappearance of the whole quantity of air after the laſt inflammation.

3. "The inflammable air has been really produced from the water; for if the electric fluid had in any other manner contributed to the formation of the airs thus obtained, it would not have furniſhed, under the ſame circumſtances, two ſpecies of air ſo diſſimilar as the dephlogiſticated and the inflammable airs. This laſt mentioned air therefore is to be entirely attributed to the water.

4. "The dephlogiſticated air muſt likewiſe derive its origin from the decompoſition of the water, for if it were owing to ſome air accidentally contained in the water, it would not be purer than common air, which contains only a certain portion of dephlogiſticated air. In that caſe, the reſiduum could not have been diminiſhed by the ſucceſſive inflammations, nor would it have entirely diſappeared.

5. "Laſtly,

5. " Laſtly, As the air obtained from the water by means of electricity, is a mixture of inflammable and dephlogiſticated air, and as the inflammation converts this air again into water, the ſame experiments afford a ſynthetick, as well as an analytick, demonſtration, of the theory advanced concerning water. It naturally follows from thoſe experiments, that the portion of acid, which has always been found together with water after the uſual combuſtion of inflammable and dephlogiſticated air, from which the opponents of the new theory have derived a ſtrong argument in their favour, muſt be conſidered as an accidental production, which cannot poſſibly invalidate the propriety of the conſequences deduced from the experiments.

" The adherents to the new theory have long ſince explained, in a very ſatisfactory manner, the formation of this acid, which is known to be the nitrous, in the combuſtion of the two airs; by attributing it to a combination of dephlogiſtated air with ſome phlogiſticated air, which may always be ſuppoſed to exiſt in dephlogiſticated air, as this

this ſort of air cannot be obtained perfectly pure by any known method. As ſome difficulties have been ſtarted againſt this theory, we think it not improper to add a few ſhort obſervations in reply, though they may not be immediately connected with the principal ſubject of this letter.

" It has been objected, 1ſt, That the purer the reſiduum is after the combuſtion, (which ſhews that the dephlogiſticated air employed muſt have been purer and freer from phlogiſticated air) the ſtronger is the acidity of the liquor obtained; and on the contrary, that when the reſiduum contains more phlogiſticated air, the liquor which is obtained approaches nearer to the nature of pure water. 2d, That by adding phlogiſticated air to the mixture of the two airs, the acidity of the liquor is diminiſhed; ſo that if inſtead of dephlogiſticated air, common air be uſed, which contains a conſiderable portion of phlogiſticated air, the acidity will hardly be perceivable.

" In order to explain the firſt difficulty we muſt obſerve, that the dephlogiſticated air

air has a greater affinity to inflammable than to phlogiſticated air, and that it never combines with the latter, unleſs it cannot meet with a quantity of inflammable air ſufficient for its ſaturation. Let us therefore conſider the differences which muſt ariſe, from uſing various proportions of inflammable and dephlogiſticated air.

" Let the quantity of inflammable air be invariably the ſame, and let the dephlogiſticated air vary in quantity; we muſt likewiſe ſuppoſe that the latter does always contain a portion of phlogiſticated air, which, by combining with the dephlogiſticated air, may form the nitrous acid, from whence the liquor obtained derives its acidity.

1. " If the quantity of dephlogiſticated air be not greater than what may be fully combined with the inflammable air, the liquor will conſiſt of nothing but pure water, and the reſiduum will be phlogiſticated air.

2. " If there be an exceſs of dephlogiſticated air, the ſuperfluous quantity of it will

will combine with the phlogiſticated air, and with it will form the nitrous acid. The liquor thereby obtained will conſiſt of water and acid; and the remainder, if there be any, will conſiſt of nearly pure dephlogiſticated air.—Both thoſe particulars are confirmed by Mr. Cavendiſh's experiments.

3. "The ſecond objection is of an equivocal nature; for if by the addition of phlogiſticated air, it be meant, that the mixture of the two airs is increaſed by the addition of phlogiſticated air, it is evident that this addition cannot occaſion any alteration in the reſult, becauſe when the dephlogiſticated air meets with a great quantity of inflammable air, it will not combine with its own portion of phlogiſticated air, and of courſe it will much leſs combine with the phlogiſticated air that has been added. This is the reaſon why Dr. PRIESTLEY's experiments, when he added phlogiſticated air to the mixture of the two airs, were not attended with any peculiar effect. The ſame reaſoning muſt be adopted when atmoſpheric air is uſed inſtead of dephlogiſticated air; for when the quantities are alike,

alike, either no particular effect will take place, or the acidity of the liquor will be very slight, and the residuum will be more impure. If, on the contrary, the phlogisticated air be added, and at the same time the quantity of inflammable air be diminished, whilst that of the dephlogisticated air remains the same; a portion of the latter, becoming superfluous, will combine with the phlogisticated air, and with it will form the acid. The residuum itself may, in great measure, consist of dephlogisticated air. Such has been the result of Mr. CAVENDISH's experiments, who has proceeded in the above-mentioned manner. It appears that for this reason such results have been totally different from those of Dr. PRIESTLEY.

"We must confess that there remains to be explained, why the combination of dephlogisticated and phlogisticated air takes place in the said combustion; whilst the electric shock is the only means by which that combination can be effected. We have felt the force of this difficulty too late, and when it was no longer in our power to clear

it by a particular ſet of experiments; but we are led to believe, that the cauſe of it muſt be attributed to the great quantity of light which is manifeſted at the time of the inflammation. Mr. Bertholet has diſcovered, that the nitrous acid, when expoſed to the light, yields dephlogiſticated air. We have obſerved this effect of light, not only upon the nitrous, but likewiſe on the vitriolic acid, when expoſed to the focus of a burning-glaſs; and we have obſerved the ſame effect, when the electric ſhock has been applied to thoſe acids. Having therefore remarked this analogy between light and electricity in the decompoſition of acids, it ſeems probable, that the light manifeſted by the inflammation may produce an effect analogous to that of electricity, in the formation of nitrous acid, viz. to produce that acid by the combination of dephlogiſticated and phlogiſticated air.

"After a review of thoſe obſervations, which convert one of the ſtrongeſt objections to the new theory concerning the nature of water, into an argument in favour of the ſame theory; there ſeems to remain no

doubt with respect to the nature of water; and we may conclude with saying, that it consists of the bases of dephlogisticated and phlogisticated air.

"We are, &c."

It is necessary to add, for the satisfaction of my readers, that most of the valuable experiments which are mentioned in the preceding letter, having been repeated in London, have been found to answer in the manner above stated with all the accuracy which can be expected in such cases. Some of the observations might perhaps require farther elucidation and investigation; but this examination would lead us too far into the doctrine of permanently elastic fluids, which is foreign to the subject of this work.

OF THE

# REPULSION

BETWEEN BODIES POSSESSED OF THE SAME SORT OF

# ELECTRICITY;

AND

Of fome Experiments, which feem to militate againft the Theory of a fingle Electric Fluid.

THE principal phenomenon, which, in the opinion of many writers, cannot be explained by the Franklinian theory of a fingle electric fluid, is the repulfion which takes place between bodies negatively electrified; and, notwithftanding what has been faid about it in the preceding editions of my Treatife, I find that in fome recent publications the faid repulfion is ftill fuppofed to contradict that theory. It feems therefore neceffary to examine and to explain that

that phenomenon in the following more particular manner:

Propofition I.—*No electricity can appear on the furface of a body, or no body can be electrified either pofitively or negatively, unlefs the contrary electricity can take place on other bodies contiguous to it.*

This propofition may be proved by a great many experiments and obfervations, fuch as may be feen in Chap. V. and VI. Part I.; and in pages 199, 201, 212, 248, 331, of Vol. I.; alfo in page 247, &c. of Vol. II. of this work.

Propofition II.—*There is fomething on the furface of bodies which prevents the fudden incorporation of the two electricities, viz. of that poffeffed by the electrified body, with the contrary electricity poffeffed by the contiguous air, or other furrounding bodies.*

Without examining the nature, the extent, and the laws of this property in bodies, it will be fufficient for the prefent purpofe to obferve, that the fact is certainly

ſo; for otherwiſe a body could not poſſibly be electrified, or it would not remain electrified for a ſingle moment.

Propoſition III. — *Suppoſing that every particle of a fluid has an attraction towards every particle of a ſolid; if the ſolid be left at liberty in a certain quantity of that fluid, it will be attracted towards the common centre of attraction of all the particles of the fluid.*

Let the body be extremely ſmall, and it is evident that it muſt be drawn towards the common centre of attraction; for if it be placed on one ſide of the ſaid centre, the attractive particles on the oppoſite ſide, being more numerous, will naturally draw it that way. If the body be large, the ſame reaſoning ſhews that the effect muſt be the ſame; for the attractive force of all its particles being concentred in a point or centre, will draw that centre towards the centre of attraction of all the particles of the fluid. Thus, if the fluid be of a ſpherical form, and the ſolid body be likewiſe ſpherical,

rical, the centre of the latter will coincide with that of the former.

Corollary 1.—The ſame thing muſt happen, when the quantity of fluid is ſmaller than the bulk of the body; in which caſe the former muſt be within the latter.

Corollary 2. — If the attraction of the particles of the fluid be exerted only towards the ſurface of the ſolid, and not towards its internal parts, the effect will be the ſame; when the body is of a regular ſhape, as ſpherical, cubical, &c. but with very irregular ſhapes, the difference will be very trifling, and not deſerving of notice in this place.

The application of the foregoing propoſitions, in explanation of the repulſion which takes place between bodies poſſeſſed of the ſame ſort of electricity, is very eaſy, and, in my opinion, concluſive. According to the Franklinian hypotheſis, the electric fluid is elaſtic or repulſive of its own particles, and attractive of the particles of other matter. Let then A and B, fig. 7, be two ſpheres

of metal ſuſpended in the open air contiguous to each other, and capable of being eaſily moved. Let ſome electricity be communicated to them, and by Propoſition I. it will appear, that whilſt the bodies touch each other, as ſhewn in fig. 7, the electricity which is communicated cannot be diſperſed equably all over their ſurfaces, but it muſt be thicker or more condenſed on the parts that are remote from the mutual point of contact, becauſe there the air is at liberty to acquire a contrary electricity; whereas near the point of contact the electricity cannot be manifeſted, becauſe in that place there is no air or other body that can acquire the contrary electricity. Therefore the atmoſpheres of contrary electricity cannot be concentric with the ſpheres A and B; but muſt be ſituated as in fig. 7. It follows therefore by Propoſition III. that the ſpherical bodies being attracted towards the centres of thoſe atmoſpheres, appear to repel each other, as ſhewn in fig. 8; ſo that when the bodies are electrified poſitively, in which caſe, according to the hypotheſis, they have acquired an additional quantity of electric

 fluid,

fluid, negative atmoſpheres will be formed round them, and the additional or ſuperfluous electric fluid of the bodies will attract, and be attracted by, thoſe negative atmoſpheres. And when the bodies are electrified negatively, in which caſe, according to the hypotheſis, they have loſt part of their uſual quantity of electric fluid, poſitive atmoſpheres will be formed round them, which will attract the under-charged bodies.

What has been exemplified in the two metallic bodies, may be eaſily applied to explain the electrical repulſion between any number of bodies, and to bodies of any ſhape and ſubſtance; proper allowance being made for their conducting or non-conducting nature, and for their weight, which may render them more or leſs ſuſceptible of the action of electricity.

The other experiments, which have been conſidered as repugnant to the Franklinian hypotheſis, may be almoſt all reduced to this, viz. that in making the diſcharge of a Leyden phial, not highly charged, through a

long circuit, the effect of the discharge is more sensibly felt by those parts of the circuit which lie near the two coatings of the phial, than by the middlemost parts of the circuit which lie farther off. But very little consideration is required, to shew that the explanation of that phenomenon, upon the above-mentioned hypothesis, is not attended with any difficulty. I shall, however, for the sake of perspicuity, describe two or three of those experiments, and shall afterwards subjoin a general explanation.

Experiment 1.—Charge a Leyden phial very weakly, viz. so as just to afford a visible spark when discharged; then apply a finger of one hand to the outside, and a finger of the other hand to the inside coating of it. The effect will be, that the slightest pricklings will be felt on these fingers, and no where else. Charge the phial a little higher, then apply the fingers to its two coatings as before, and a sharper sensation will be felt all along the two fingers. If the phial be charged still higher, the sensation will be felt as high as the

wrists;

wriſts; with a greater charge, the ſenſation will be felt in the arms, &c.

Experiment 2.—Inſulate a great number of metallic balls or bars, or, in ſhort, many conducting ſubſtances, and diſpoſe them ſo as to be within a ſmall diſtance of each other, but not in actual contact. The diſtances between any two contiguous bodies ought to be equal, which may be eaſily done by interpoſing a card, or ſomething elſe of a proper thickneſs, when the bodies are ſituated in their places, but not to remain between them. Let this interrupted circuit form the communication between the two ſides of a charged Leyden phial; and it will be found, that when the charge of the phial is very weak, ſparks will be ſeen between thoſe interruptions of the circuit which are near the two coated ſurfaces of the phial; if the charge be higher, the ſparks will be extended through more interruptions; and if the charge be ſtill higher, the ſparks will be ſeen through all the interruptions of the circuit.

Experiment 3.—Take a pretty long glaſs tube furniſhed with metallic caps at both ends, one of which caps muſt have a ſtop-cock, and exhauſt it of air by means of an air-pump. Then let this exhauſted tube form part of the circuit between the inſide and outſide of a charged Leyden phial; and it will be found, that when the charge of the jar is very weak, the two ends only of the tube will be illuminated; whereas when the charge is ſufficiently high, the light will pervade the whole cavity of the tube from end to end.

In theſe and many ſimilar experiments, thoſe perſons who wiſh to find fault with the hypotheſis of a ſingle electric fluid, imagine that a double current, and two diſtinct powers, muſt neceſſarily exiſt. They are ſatisfied with the ſlight evidence of their ſenſes, and do not give themſelves the trouble of conſidering the matter any farther.

In order to ſhew, that thoſe appearances are perfectly and unequivocally explainable on the theory of a ſingle electric fluid, I ſhall

ſhall juſt mention two known truths, viz. 1ſt, That the condenſation of an elaſtic fluid, ſuch as the electric fluid is ſuppoſed to be, is inverſely as the ſpaces in which it is confined. Thus, when a certain quantity of it is confined in half the ſpace, it is then ſaid that its condenſation is double of what it was before; when it is confined into the tenth part of the original ſpace, the condenſation is ten times greater, and ſo on. 2dly, That the effects produced by a certain quantity of electricity, ſuch as the ſpark, ſhock, &c. are proportional to its condenſation. Thus, the higheſt charge of a pint phial will give a man a much greater ſhock than he would wiſh to receive; yet if that charge be communicated to a battery of one hundred ſquare feet, and the ſame man apply his hands to the two ſides of that battery, he will hardly feel the ſhock, becauſe the ſame quantity, which in the former caſe was confined into a ſmall ſpace, loſes the greateſt part of its power when it comes to be rarefied into a much larger ſpace.

Thus much having been premiſed, let us conſider the ſecond experiment, and let

us

us ſuppoſe that the inſide and outſide coated ſurfaces of the Leyden phial are each equal to five ſquare inches; that before the charge, each of theſe ſurfaces contained five parts of electric fluid, viz. one for every ſquare inch; but after the charge, the inner ſurface acquired four additional parts of it, ſo as to make all together nine, and the outſide loſt four of its original number, in conſequence of which one part only of electric fluid remained in it. We muſt likewiſe ſuppoſe, that each of the bodies which form the interrupted circuit, has a ſurface of one ſquare inch, and contains one part of electric fluid. Now, when the two coatings of this charged phial come in contact with the extremities of the interrupted circuit, the inſide endeavours to diſcharge its four parts of ſuperfluous electric fluid, and the outſide endeavours to recover its four parts of loſt electric fluid. By the contact of the body next to it, the inſide of the phial, together with that body, will form a ſurface of ſix ſquare inches, and the whole quantity of electric fluid contained in it, is ten parts; but ten parts of fluid will have more room in ſix ſquare inches, than nine parts

parts had in five ſquare inches; therefore the electric fluid is leſs condenſed now than it was before the contact of the firſt body. Further, when the electric fluid has reached the ſecond body of the circuit, the whole quantity of fluid will be eleven parts, and the whole quantity of ſurface will be ſeven ſquare inches; but eleven parts of fluid in ſeven inches of ſurface, will have more room than ten parts in ſix inches of ſurface; therefore by the contact of the ſecond body of the circuit, the electric fluid becomes ſtill more rarefied, and ſo on; the condenſation therefore, and, of courſe, the intenſity of the ſpark and other effects, diminiſh in proportion as the electric fluid proceeds in the circuit farther and farther from the coated ſurface of the Leyden phial.

If the ſame reaſoning be applied to the outſide negative ſurface of the phial, it will in the like manner appear, that the attraction of that ſurface towards the electric fluid, is diminiſhed in proportion as the bodies of the circuit ſucceſſively add their natural ſhare of fluid to the joint ſurface, viz. the

the ſurface of the phial, together with their own ſurface. Therefore when the Leyden phial is weakly charged, the ſparks will appear only between thoſe bodies which are contiguous to its ſurfaces, or, in general, the effects of the diſcharge are more remarkable near the phial, and are leſs conſpicuous at a greater diſtance from it.

The peculiar merits of different hypotheſes on any philoſophical ſubject whatſoever, are not to be derived from thoſe phenomena which admit of an eaſy explanation upon different ſuppoſitions, but they muſt be determined by thoſe which can only be explained upon one theory, and no other. Therefore the foregoing experiments will not render the hypotheſis of a ſingle electric fluid more probable than ſome other electric hypotheſis; but the experiments which render that hypotheſis more likely to be true, are thoſe which ſhew an evident current from the poſitive to the negative ſide in the diſcharge of a Leyden phial, or, in general, a current from a body poſitively electrified, and towards a body negatively electrified;

electrified *; since those experiments do not admit of an easy explanation on other hypotheses. Therefore the object of this section has been only to shew, that those experiments which are considered as contradictory to the theory of a single electric fluid, are so far from contradicting it, that they may be clearly and satisfactorily explained by it.

* The reader will find many experiments of this sort in the first and second volumes of this Treatise.

## REMARKS

### ON SOME EXTRAORDINARY EFFECTS OF

### THUNDER STORMS;

### AND AN EXPLANATION OF THE

### ELECTRICAL RETURNING STROKE.

ONE of the moſt inſtructive accounts of thunder ſtorms that have appeared in print, was drawn up ſome years ago by PATRICK BRYDONE, Eſq; F.R.S. and was publiſhed in the 77th volume of the Phil. Tranſ. The remarkable effects contained in that account, and the accurate manner in which they were obſerved and deſcribed, renders it ſo highly intereſting to electricians, that I ſhall tranſcribe the principal part of it in this place, and ſhall then ſubjoin the beſt conjecture which has been hitherto offered in explanation of the phenomena therein contained.

"Tueſday

" Tuefday the 19th of July, 1785, was a fine foft morning (thermometer at ten, 68°.); about eleven, clouds began to form in the fouth-eaft; and between twelve and one there were feveral flafhes of lightning, followed by rolling claps of thunder at a confiderable diftance. I was fitting in my ftudy at an open window, in the fecond ftory, obferving the progrefs of the ftorm; when fome ladies, who were in the drawing-room below, alarmed by the lightning, came up to me. I was making them obferve, by a ftop-watch, the time which the found took to reach us (which was generally from 25 to 30 feconds) and affuring them the ftorm was at fo great a diftance, that there could be no fort of danger; when we were fuddenly alarmed by a loud report, for which we were not prepared by any preceding flafh: it refembled the firing of feveral mufkets, fo clofe together, that the ear could hardly feparate the founds, and was followed by no rumbling noife like the other claps.

" The clouds immediately began to diffipate, and there was no more appearance

 of

of either thunder or lightning. I had ordered my horſes to be got ready, and was juſt going to mount, when a ſervant came running in to tell me, that a man and two horſes had been ſtruck dead by the thunder, at a ſmall diſtance from the houſe. I immediately ſet out, and arrived on the ſpot in leſs than half an hour after the accident. The horſes were ſtill yoked to the cart, and lying in the ſame poſition in which they had been ſtruck down; but the body of the young man had been already carried off by his companion, who ſoon returned to the place; and, with leſs agitation than I expected, deſcribed to me how every thing had paſſed.

"They were both ſervants to Mr. Turnbull, a tenant of the Earl of Home, and were returning home with two carts loaded with coals. James Lauder, a ſtrong young man, of 24 years of age, had the charge of the firſt cart, and was ſitting on the fore-part of it. They had croſſed the Tweed a few minutes before, at a deep ford, and had almoſt gained the higheſt part of an aſcent, about 65 or 70 feet above

above the bed of the river. They were converſing about the thunder, which they heard at a diſtance, and expreſſing a wiſh that it might be accompanied by a fall of rain, as the only means of ſaving the crop, after ſo long and ſo ſevere a drought. At that inſtant he was ſtunned by a loud report, and ſaw his companion, his horſes and cart, fall to the ground. He immediately ran to his aſſiſtance, but found him quite dead. His face, he ſaid, was of a livid colour, his clothes were torn to pieces, and he had a ſtrong ſmell of burning. He immediately emptied his own cart, and carried home Lauder's body to his friends; ſo that I had not an opportunity of examining it: but Mr. Bell, Miniſter of Coldſtream, a gentleman of the moſt perfect candour and veracity, told me, that he had been ſent for, to announce the fatal event to the young man's parents, and had examined the body: that he found the ſkin of the right thigh much burnt and ſhrivelled, and many marks of the ſame kind over the whole body, but none on the legs, which he imputed to their hanging over the fore-part of the cart at the time of the exploſion, and not being in contact

with any part of it. His clothes, and particularly his ſhirt, was very much torn, and emitted a ſtrong ſmell of burning. The body was buried two days after, without having diſcovered any ſymptoms of putrefaction.

" Lauder's companion ſhewed me the diſtance between the two carts, which was exactly marked; for his horſes had turned round at the time of the exploſion, and broke their harneſs: I found it about 24 yards, and Lauder's cart was a few feet higher on the bank, but had not yet reached the ſummit. He told me, he was likewiſe ſitting on the fore-part of his cart, and had Lauder, his cart and horſes, full in view, when they fell to the ground; that he perceived no flaſh nor appearance of fire, and was ſenſible of no ſhock, nor uncommon ſenſation.

" I now examined the cart, and the ſpot round it, as exactly as I could. The horſes were black, and of a ſtrong make; they had fallen on the left ſide, and their legs had made a deep impreſſion in the duſt, which,

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on our lifting them up, ſhewed the exact form of each leg; ſo that no kind of ſtruggle or convulſive motion had ſucceeded the fall, but every principle of life ſeems to have been extinguiſhed in an inſtant. The hair was much ſinged over the greateſt part of their bodies, but was moſt perceptible on the belly and legs. Their eyes were already become dull and opake, and looked like the eyes of an animal which had been long dead. The joints were all ſupple; and I could not perceive that any of the bones were either ſoftened or diſſolved, as it has been alledged ſometimes happens to animals killed by lightning. The left ſhaft of the cart was broken; and I obſerved, that ſplinters had been thrown off in many places, particularly where the timber of the cart was connected by nails, or cramps of iron. Many pieces of the coal were likewiſe thrown out to a conſiderable diſtance, all round the cart; and ſome of them which I have preſerved, have the appearance of coal which had lain ſome time on a fire. I likewiſe gathered up the fragments of Lauder's hat, which had been torn to innumerable ſmall pieces; ſome of which I

 ſhall

ſhall incloſe for your inſpection, as well as part of his hair, which I found ſtrongly united to ſome of the fragments which had compoſed the crown of the hat. About four feet and a half behind each wheel of the cart, I obſerved an odd appearance in the ground; a circular hole of about twenty inches in diameter, the centre of which was exactly in the track of the wheel. The earth was torn up as if by violent blows of a pick-axe, and the ſmall ſtones and duſt were ſcattered on each ſide of the road. The tracks of the wheels were ſtrongly marked in the duſt, both behind and before theſe holes, but were completely obliterated for upwards of a foot and a half on theſe ſpots. This led me to ſuſpect, that the force which had formed them, muſt likewiſe have acted ſtrongly upon the wheels; and, on examination, I found evident marks of fuſion on each of them, which I now ſhewed to many people who had aſſembled around us. The ſurface of the iron, to the length of about three inches, and the whole breadth of the wheel, had become of a bluiſh colour, had entirely loſt its poliſh and ſmoothneſs, and had the appearance of drops

drops incompletely formed on its ſurface; theſe were of a roundiſh form, and had a ſenſible projection. I ſuſpected that the great heat which had been communicated to the iron, might probably have burnt the wood of the wheels; but this I did not find to be the caſe. To aſcertain whether theſe marks were occaſioned by the exploſion which had torn up the ground, we puſhed back the cart on the ſame tracks which it had deſcribed on the road; and found, that the marks of fuſion anſwered exactly to the centre of each of the holes; and that, at the inſtant of the exploſion, the iron of the wheels had been ſunk deep in the duſt. They had made almoſt half a revolution after the exploſion, which might be occaſioned by the falling down of the horſes, which pulled the cart a little forward. On examining the oppoſite part of the wheels, or that part which was at the greateſt diſtance from the earth, no mark of any kind was obſervable. The broken earth ſtill emitted a ſmell ſomething like that of ether. The ground was remarkably dry, and of a gravelly ſoil.

"It would appear, that this great explosion had, in an inſtant, pervaded every ſubſtance connected with the cart, the wheels of which had probably conducted it from the ground. They had been completely wetted but a few minutes before, as well as the legs and bellies of the horſes, and this might, perhaps, be the reaſon why the hair on theſe parts was much more burnt than on the reſt of their bodies. However, the two horſes had already walked over this electrical mine, without having produced any effect; and had not the cart followed them might have eſcaped without hurt. I examined all their ſhoes, but could not perceive the leaſt mark on any of them, nor was the earth broken where they had trodden; but the cart was deeply laden, and the wheels had penetrated much farther into the ground.

"The equilibrium between the earth and the atmoſphere ſeems at this inſtant to have been completely reſtored; for no farther appearance of thunder or lightning was obſerved within our hemiſphere; the clouds diſpelled, and the air reſumed the

moſt perfect tranquillity: but how this vaſt quantity of electric matter could be diſcharged from the one element into the other without exhibiting any appearance of fire, I ſhall not pretend to examine. The fact, however, appears certain, and when I was mentioning it as a ſingular one, a gentleman told me, that the ſhepherd of St. Cuthber's farm, on the oppoſite bank of the Tweed, had been an eye-witneſs of the event, and gave a different account of it. I immediately went to the farm, found the ſhepherd, and made him conduct me to the ſpot from whence he has obſerved it, and deſired him to give me an account of what had happened. He was looking, he ſaid, at the two carts going up the bank, when he was ſtunned by a loud report, and at the ſame inſtant ſaw the firſt of the carts fall to the ground, and obſerved that the man and horſes lay ſtill, as if dead. I aſked him, if he had obſerved any lightning? He ſaid, he ſaw no lightning, nor appearance of fire whatever; but obſerved the duſt to riſe at the place; that there had been ſeveral flaſhes of lightning ſome time before from the ſouth-eaſt, whereas the accident

happened to the north-weſt of where he ſtood. The diſtance, in a right line acroſs the river, might be between two and three hundred yards. He was ſenſible of no ſhock, nor uncommon ſenſation of any kind. I went next morning to examine if there were any marks of putrefaction on the horſes, and to obſerve the ſtate of the blood-veſſels, &c. after the ſkin had been taken off; but a gentleman of the neighbourhood, who kept a pack of hounds, had already ſeized on them.

"Several other phenomena happened on that day, probably all proceeding from the ſame cauſe; ſome of which I ſhall beg leave to mention.

"The ſhepherd, belonging to the farm of Lennel Hill, was in a neighbouring field, tending his flock, when he obſerved a lamb drop down, and ſaid, he felt at the ſame time as if fire had paſſed over his face (this was his own expreſſion) although the lightning and claps of thunder were then at a great diſtance from him. He ran up immediately, but found the lamb quite dead; nor did he

he perceive the leaſt convulſive motion, nor ſymptom of life remaining, although the moment before it appeared to be in perfect health. He bled it with his knife, and the blood flowed freely. This, he told me, happened about a quarter of an hour before the exploſion which killed Lauder; and it was not above 300 yards diſtant from the ſpot. He was only a few yards from the lamb when he fell down. The earth was not torn up, nor did he obſerve any duſt riſe.

"Thomas Foſter, a celebrated fiſher in Coldſtream, and another man, were ſtanding in the middle of the Tweed, fiſhing for ſalmon with a rod, when they ſuddenly heard a loud noiſe; and, turning round to ſee from whence it came, they found themſelves caught in a violent whirlwind, which Foſter told me felt ſultry and hot, and almoſt prevented them from breathing. It was not without much difficulty they could reach the bank, where they ſat down, exhauſted with fatigue, and greatly alarmed: however, it laſted but a very ſhort time, and was ſucceeded by a perfect calm. This happened

happened about an hour before the explosion.

"A woman, making hay near the banks of the river, fell ſuddenly to the ground, and called out to her companions, that ſhe had received a violent blow on the foot, and could not imagine from whence it came. This I had not from the woman herſelf, but from Mr. Turnbull, a very reſpectable farmer. Mr. Bell, our Miniſter, nephew of Thomſon the poet, and poſſeſſed of all the candour and ingenuity of his uncle, told me, that, walking in his garden, a little before Lauder's accident, he ſeveral times felt a ſenſible tremor in the ground. He likewiſe told me (what I find I had forgot to mention in the proper place) that he had obſerved on Lauder's body a zig-zag line of about an inch and a quarter broad, which extended from his chin down to his right thigh, and had followed nearly the line of the buttons of his waiſtcoat. The ſkin was burnt white and hard.

"Theſe, Sir, are all the circumſtances I have been able to collect that are well authenticated;

thenticated; and I ſhall not trouble you with reports that are not. From the whole it would appear, that the earth had acquired a great ſuperabundance of electrical matter, which was every where endeavouring to fly off into the atmoſphere. Perhaps it might be accounted for from the exceſſive dryneſs of the ground; and, for many months, the almoſt total want of rain, which is probably the agent that Nature employs in preſerving, or in reſtoring, the equilibrium between the other two elements. But I ſhall not pretend to inveſtigate the cauſes: all I wanted, was to give you ſome account of the effects, and your own reflections will lead you much farther than any thing I could ſuggeſt."

After an attentive conſideration of this account, ſeveral of the appearances obſerved in it, ſeem to furniſh conſiderable inſtruction, and at the ſame time to require an attentive inveſtigation of the cauſes from which they are likely to have originated. The moſt ſingular circumſtance is, that notwithſtanding the greatneſs of the effect, though a man and two horſes were ſtruck dead

dead by the matter of lightning, yet this great diſcharge of electrical matter was not attended with any appearance of light, no flaſh of lightning was obſerved by ſpectators, who, it ſeems, ought to have unavoidably perceived it, if there had been any. Whether the ſuperabundance of electric fluid exiſted in the clouds or in the earth, if the diſcharge was made through the air, a very intenſe light would have certainly accompanied it; and if the diſcharge was not made through the air, after it had gone through the man and horſes, how could it be attended with ſo loud a report as the witneſſes aſſert to have heard at the moment of the accident?

The other phenomena, related at the end of the account, did alſo happen without any appearance of light; at leaſt it is not mentioned that there was any, and we may preſume, that it would have been noticed if there had. However, in the affair of the lamb, the ſhepherd who ſaw it drop, ſaid, that he felt at the ſame time as if fire had paſſed over his face; but from this indefinite expreſſion we cannot clearly under-

ſtand,

ſtand, whether what he perceived was heat, light, or both heat and light together.

That the earth about that ſpot was charged with electrical matter, and that all the phenomena related in the account were effected by that cauſe, ſeems not to admit of a doubt; yet the ſuperincumbent clouds had great ſhare in the production of thoſe phenomena; and they either produced the accumulation of electric matter in that part of the earth by the action of their electric atmoſpheres, or they themſelves were drawn near, and became inſtrumental in the reſtoration of the electrical equilibrium.

The famous Italian philoſopher, Fr. Beccaria, was of opinion, that very great quantities of electric matter were ſometimes diſcharged from the earth in ſome particular ſpots, and, ruſhing into the atmoſphere, drove in their way ſand, aſhes, leaves of trees, and other light matters; and that without the interference of any wind, or ſometimes even againſt the direction of the wind, as he had more than once obſerved.

observed. But independent of this phenomenon, he was led to suppose, that the earth became over-charged with electricity in some particular places, and undercharged in others, and that the equilibrium between those differently electrified places was restored through the clouds; he being otherwise unable to account for the vast quantities of electricity, which were successively discharged, for a considerable length of time, from clouds of a limited extent.

Had the production of electricity from evaporation, and from condensation, been known by Fr. Beccaria, he would perhaps have not wondered at the successive production of lightning from clouds in a thunder-storm; for the clouds must not be considered as conductors simply electrified, which remain less and less electrified in proportion as sparks are drawn from them; but they acquire electricity from their very condensation or rarefaction, and this production of electricity, being proportionate to the quantity and quickness of the condensation or rarefaction, must be very great in certain cases, especially when the clouds are quickly converted

converted into rain, which is generally the caſe in thunder-ſtorms.

Notwithſtanding this obſervation, it is not unlikely but that Fr. Beccaria's hypotheſis may ſometimes be true, viz. that one part of the earth may become over-charged, whilſt another part becomes under-charged, and that the clouds may form a ſort of conducting rod between thoſe two places*; yet even upon Fr. Beccaria's ſuppoſition, the above-mentioned facts do not ſeem to admit of an explanation; the want of light being irreconcileable to that hypotheſis.

A ſhort time after the reading of Mr. Brydone's account of the thunder-ſtorm in Scotland at the Royal Society, Earl Stanhope preſented a paper to the ſame ſociety, containing remarks on the ſaid account †. In that paper, Lord Stanhope endeavours to account for the various phenomena obſerv-

* Fr. Baccaria ſuppoſed, that earthquakes were produced by the accumulation of vaſt quantities of electricity deep in the bowels of the earth.

† Phil. Tranſ. vol. 77th.

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ed during the thunder-ſtorm, and attributes them to what he calls the *electrical returning ſtroke.* But before we enter into an examination of his lordſhip's explanation of the phenomena, I ſhall avail myſelf of this opportunity to give my readers an idea of the nature of this returning ſtroke.

When an inſulated body A is brought within the action of another body B, which is electrified either poſitively or negatively, but not within the ſtriking diſtance; the body A will have the equilibrium of its electric fluid diſturbed, in ſuch a manner as to acquire the electricity contrary to that poſſeſſed by B, on that ſide which is neareſt to B; but on the oppoſite ſide, it will acquire the ſame ſort of electricity; and in a certain part between thoſe two extremities the body will remain in a neutral ſtate, viz. not electrified *. On removing the electrified

* Suppoſe that the inſulated body A is a cylinder, placed with its axis in the direction of the electrified body B. Call that end of the electrified body which is neareſt to the inſulated body, F; call that end of the inſulated body which is towards the electrified body, G; and call the remoteſt end of the ſame inſulated body, H. Then Lord

electrified body B, A will recover its equilibrium, and appear unelectrified as before.

When instead of one body, two bodies, A and D, are separately insulated at a small distance from each other in the same direction with the electrified body B, and are so disposed that A may be just out of the striking distance of B, the effect will be, that by the action of the electric atmosphere of B, the body A will acquire the contrary electricity, and the body D will acquire the same sort of electricity as that of B; but this effect cannot take place unless a spark or sparks take place between the two bodies A and D. And if the electrified body B be removed, or its electricity be discharged, another spark will appear between A and D, which will restore those two bodies to their original unelectrified state.

Lord Stanhope has demonstrated, that if the density of the electricity of electrical atmospheres be inversely as the squares of the distance, the *neutral*, or unelectrified, point in the insulated body is a fourth point of an harmonical division of the line F G H, the other three points F, G, and H being given.—Principles of Electricity, Part V.

This ſpark is called by Lord Stanhope the *electrical returning ſtroke* *. It is hardly neceſſary to ſubjoin, that when the body B is electrified poſitively, the returning ſtroke conſiſts of the electric fluid, which originally belonged to A, but by the action of the electrified body B, having been driven into D, is now returned to its original place in A. But when B is electrified negatively, then the electric fluid of D is firſt driven into A, and then returns to its original place in D.

What we have exemplified with the two bodies may take place amongſt any number of bodies, ſome or the greateſt number of which being or not being inſulated, as it may be eaſily conceived without any farther explanation.

Thus much having been premiſed, we may now add Lord Stanhope's explanation of the phenomena that attended the ſtorm. His lordſhip firſt ſhews, that Lauder's

* See Part the 7th of Lord Mahon's Principles of Electricity.

death,

death, &c. could not be occaſioned *by any direct main ſtroke of exploſion*; ſince there was no lightning; and that, for the ſame reaſon, it could not be occaſioned *by any tranſmitted main ſtroke of exploſion*; and, conſequently, that it could neither be occaſioned by *a lateral exploſion*. But, " I think," *ſays he*, " from the different circumſtances of this caſe, that the effects produced proceeded from electricity; and that *no electrical fire did paſs* immediately either from the clouds into the cart, &c. or from the cart, &c. into the clouds.

" It is evident that the electrical fire did paſs (from the earth to the cart, or from the cart to the earth) through that part of the iron of the wheels, which was in contact with the ground.

" From the ſplinters that had been thrown off in many places, particularly where the timber of the cart was connected by nails or cramps of iron, and from the various other effects mentioned in Mr. Brydone's paper, it is moreover evident, that there was a violent motion of the elec-

trical fluid in all, or (at leaſt) in different parts of the cart, and of the bodies of the man and horſes, although there was no lightning.

"Wonderful as theſe combined facts may appear, and uncommon as they certainly are in this country, they are, nevertheleſs, eaſy to be explained by means of that particular ſpecies of electrical ſhock, which I have diſtinguiſhed in my Principles of Electricity (publiſhed in 1779) by the appellation of the *electrical returning ſtroke*; and although at the time I wrote that treatiſe, I had it not in my power to produce any inſtance of perſons or animals having been killed in the very peculiar manner ſince related in Mr. Brydone's paper, I did however (from my experiments mentioned in that book) venture to aſſert with confidence, that if perſons be ſtrongly ſuperinduced by the electrical atmoſphere of a cloud, they may (under circumſtances ſimilar to thoſe explained in that treatiſe) receive a very ſtrong ſhock, be knocked down, or be even killed, at the inſtant that the cloud diſcharges, with an exploſion, its electricity,

electricity, whether the lightning falls near the very place where thoſe perſons are, or at a very conſiderable diſtance from that place, or whether the cloud be poſitively or negatively electrified.

"I have alſo explained, in that treatiſe, how a ſtill more ſingular effect might be produced, namely, how an exploſion, which happens in one place, may cauſe in a ſecond place (at a very conſiderable diſtance from the firſt place) a ſudden returning ſtroke, which may knock down, or even kill, perſons and animals at that ſecond place, at the ſame time that other perſons, or other animals, ſituated in a third place, that is even immediately between the firſt place where the lightning falls, and the ſecond place (juſt mentioned) when the ſhock of the returning ſtroke happens, ſhall receive no detriment whatever."

Lord Stanhope after this proceeds to explain the phenomena, and to reconcile them to his theory of the returning ſtroke. "Now," *ſays he*, "let us ſuppoſe a cloud, eight, ten, or twelve miles in length (be

the ſame more or leſs) to be extended over the ſurface of the earth.

"And let another cloud be ſituated between the above-mentioned cloud and the earth.

"Let the two clouds be ſuppoſed to be charged (for inſtance) with the ſame kind of electricity, and to be both poſitive.

"Let us further ſuppoſe, that the lower cloud be near the earth, only a little beyond the ſtriking diſtance; and let a man, cart, and horſes, be ſituated under that part of the cloud which is the neareſt to the earth.

"Now, let us ſuppoſe this cart to be aſcending an hill, and to be in the ſituation deſcribed by Mr. Brydone, namely, to have almoſt gained the higheſt part of the aſcent, and to be followed by another cart lower down the hill.

"Let us ſuppoſe alſo, that the two clouds be near each other, perpendicularly over the place where the firſt cart was ſituate.

"And

"And let the remote end of the upper cloud approach the earth, within the ſtriking diſtance, and ſuddenly diſcharge its electricity into the earth.

"Things being ſituated as above deſcribed, let us examine what conſequences muſt follow.

"Firſt, when the upper cloud diſcharges its electricity into the earth, the lower cloud muſt immediately diſcharge its electricity into the upper cloud at the place which is directly over the cart.

"This accounts for the loud report of thunder that was unaccompanied by lightning near the carts. The *report* muſt be loud, from its being near; but no *lightning* could be perceived at the place near the carts, by reaſon of the thick thunder-cloud being ſituated immediately between the ſpectator at the ſecond cart and the place between the two clouds where the lightning was."

Lord Stanhope after this ſhews what effect would be produced by the atmo-

ſphere of the ſecond cloud upon a conducting body inſulated under it. Then, *ſays he*, " but if this conducting body, on the ſurface of the earth, be not inſulated, or be but imperfectly inſulated, then the whole of ſuch body (from its being immerged in the electrical atmoſphere of the *poſitive* cloud) will become *negative*, becauſe part of the electricity of this conducting body will, in this caſe, paſs into the earth. And this conducting body will become the more *negative*, as it becomes the more deeply immerged into the denſe part of the elaſtic electrical atmoſphere of the approaching thunder-cloud.

" Now when the poſitive lower cloud (in the manner above ſtated) comes ſuddenly to diſcharge, with an exploſion, its ſuperabundant electricity into the upper cloud, then the electrical atmoſphere of the lower cloud will ceaſe to exiſt; conſequently, the electrical fluid, which had been gradually expelled into the common ſtock, from the conducting body ſituated upon the ſurface of the earth, muſt (by the ſudden removal of the ſuperinduced elaſtic electrical

electrical preſſure of the electrical atmoſphere of the thunder cloud) ſuddenly return from the earth into the ſaid conducting body, producing a violent commotion, ſimilar to the pungent ſhock of a Leyden jar in its ſenſation and effects.

"It was by ſuch a returning ſtroke that Lauder, and the horſes that he was driving, were killed, they having become ſtrongly *negative* previous to the exploſion."

The objection which naturally occurs on firſt hearing the above-mentioned explanation is, that the quantity of electric fluid uſually contained in the body of a man, in the horſes, &c. does not ſeem to be ſufficiently great to produce the deſcribed phenomena. To this Lord Stanhope replies in the following manner:

"No perſon, the leaſt verſed in the principles of electricity, can heſitate to aſſent to the propoſition, that the *electrical returning ſtroke* muſt exiſt, under circumſtances ſimilar to thoſe explained above. But it may be objected to me, that although all the

the aforesaid effects of a returning stroke might take place in a small degree, yet those effects could not have been sufficiently powerful to have killed Lauder, the horses, and the lamb, or to have melted the iron of the cart-wheels; especially, considering the small quantity of electrical fluid that is contained in the body of a man, of a lamb, or of a horse, or that is contained in any body of the size of a common cart; that is to say, considering the small quantity of electrical fluid that could, by being disturbed, have produced the *returning stroke*.

"To this objection (plausible as at first sight it may appear) I conceive I have given a complete answer in my Principles of Electricity, from section 337 to section 347 inclusively, and also from section 592 to section 606 inclusively; but it may not be improper to add a few words to what I have already said upon that part of the subject.

"No legitimate conclusion can be drawn from premises that are not proved; therefore, no person can legitimately conclude, that

that the force of a returning ſtroke muſt always be weak, when produced by the diſturbed electrical fluid of a man's body, by reaſon that a man's body contains but a ſmall quantity of electricity: for it has never been proved, that a man's body does contain only a *ſmall* quantity of electrical fluid; neither is there the ſmalleſt reaſon to believe ſuch an hypotheſis, which appears, on many accounts, to be completely erroneous. And, if that hypotheſis be erroneous, the objection to the ſtrength of an electrical returning ſtroke remains perfectly unſupported by argument.

" When a body is ſaid to be *plus* or *poſitive*, it ſimply means, that the body contains *more* electricity than it does in its unelectrified, that is to ſay, natural ſtate; but does not ſignify, that ſuch body is completely ſaturated with electricity. In like manner, when a body is ſaid to be *minus* or *negative*, it only ſignifies, that the body contains *leſs* than its natural ſhare of electricity; but does not imply, that ſuch body is *completely exhauſted* of the electricity which it contains in its natural ſtate.

" Now,

"Now, the ſtrength of natural electricity is ſo immenſe, when compared to the very weak effects of our largeſt and beſt contrived electrical machines, that I conceive, that we cannot, by means of artificial electricity, expel, from a man's body, the thouſandth (or perhaps even the ten thouſandth) part of the electrical fluid which it contains, when in its natural ſtate."

The importance of the ſubject, and the ſingularity of the effects produced by the above related thunder-ſtorm, have induced me to dwell longer upon it than I at firſt intended. Lord Stanhope's hypotheſis is ingeniouſly adapted to the explanation of the phenomena; yet as many of the data, upon which that explanation depends, are mere ſuppoſitions, ſuch as the ſituation of the clouds, &c. we can only conſider it as a probable hypotheſis, but not as a matter of demonſtration.—I ſhall beg leave to add a few remarks on the general ſubject of thunder-ſtorms.

Whoever conſiders the immenſe ſize of the electrified clouds in thunder-ſtorms, and the

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the vaſt quantity of electrical matter diſcharged by them, will eaſily allow, that the experiments performed with our electrical machines can give us but a very inadequate idea of the effects produced by the electricity of the clouds: and though we derive conſiderable inſtruction from the uſe of our machines, yet we ſhould never omit any opportunity of taking particular notice of the natural phenomena, ſince it is from them alone that we can be enabled to acquire adequate ideas of the natural operations, and to form ſuch rules as are neceſſary either to judge of the effects, or to prevent thoſe accidents, to which we are naturally expoſed in thunder-ſtorms.

It is well known, that when a large electrified cloud is near, the electrometers in the open air are powerfully affected by it; but my reader may reſt aſſured, that from my own obſervation, as well as from the teſtimony of other perſons, when a large electrified cloud is over head, the electrometers within a room, even when the windows and doors are ſhut up, are affected by the electrical atmoſphere of the cloud. And when clouds differently

differently electrified ſucceed each other, or after a clap of thunder, the change may be frequently obſerved by means of the in-door electrometers. For this purpoſe the electrometers muſt not be incloſed in bottles, and as in that caſe gold leaf electrometers cannot be uſed, it becomes neceſſary to make thoſe with cork-balls in a very nice manner. They anſwer rather better when they are made ſo that the cork-balls, when not electrified, do not actually touch, but remain at a very ſmall diſtance from each other; for when the cork-balls are in actual contact, a degree of adheſion takes place, which is not eaſily overcome by very ſmall quantities of electricity.

With reſpect to ſtrokes of lightning, I have been aſſured by more than one eye-witneſs, that ſometimes the lightning, in ſtriking a building, a tree, or other object, aſſumes the form of a bruſh, and its branches ſeem to ſtrike ſeveral points at once*. This phenomenon may be imitated by preſenting

* Its more uſual appearance is certainly of a ſingle body or ball of fire.



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a conducting body to the negative conductor of an electrical machine. By this obſervation we are enabled to underſtand, why ſometimes a building, which is furniſhed with a conductor, happens to be ſtruck by the lightning in ſome other part beſides the conductor.

In ſuch caſes the damage is never very material; for firſt, the principal ſtroke is received and carried away by the conductor; and, ſecondly, that branch of the lightning, which falls on ſome other part of the building, ſoon finds its way to thoſe conducting bodies that are connected with the principal conductor.

This obſervation does by no means invalidate the uſe of conductors for the defence of buildings from the effects of lightning; but only ſhews the neceſſity of erecting more conductors than one upon buildings that are not very ſmall, and likewiſe the precaution of connecting together, and with the conductor, all the prominent metallic bodies which may happen to be on the top of a building.

OF THE ACTION OF

## ELECTRICITY

## ON THE VEGETABLE KINGDOM.

IT has frequently happened, in almoſt every branch of natural philoſophy, that a conſiderable deal of labour has been neceſſarily exerted to eradicate wrong notions, which had been introduced by artifice or by accident, and had afterwards eſtabliſhed their reputation on the long acquieſcence of the philoſophical world. The hiſtory of Electricity has furniſhed ſeveral inſtances of this obſervation; and to that number one more will be added in this ſection.

That electricity promotes vegetation is a propoſition, which was admitted many years ago, and has at various times received apparent confirmation from the publication of

of experiments and obſervations made either accidentally, or expreſsly for the purpoſe. The extenſive influence of electricity in nature, its conſtant preſence in the atmoſphere, and its being generally found in rain, hail, ſnow, &c. corroborated the probability of its being either abſolutely neceſſary or uſeful to vegetation; but Dr. Ingen-Houſz's attentive examination of the ſubject, and the reſult of his numerous experiments, have ſhewn the fallacy of the propoſition, by having expoſed the inſufficiency of the experiments upon which it was eſtabliſhed *.

Previous to the publication of Dr. Ingen-Houſz's experiments and obſervations it was generally believed, that ſuch plants as were kept in a ſtate of conſtant electrization, would grow faſter, and appeared more luxuriant than others of the ſame ſort, which were treated in every reſpect like them, except that the latter were not elec-

* See Dr. Ingen-Houſz's two letters to Mr. Molitor, publiſhed in the *Journal de Phyſique* for February 1786, and May 1788.

trified. It had been ſaid, that certain plants which were ſituated near a conductor of lightning, grew to an uncommon ſize in conſequence of that ſituation *.—It had been likewiſe aſſerted, that ſome plants which were ſituated under ſome horizontal conductors or wires, ſuch as are fixed for the purpoſe of exploring the daily electricity of the atmoſphere, appeared ſickly, and produced little fruit, as if the conductor robbed them of the vivifying electrical principle which they received from the air; and that after removing thoſe wires, the plants ſoon became as healthy and as luxuriant as the reſt †.

Dr. Ingen-Houſz endeavoured to verify thoſe aſſertions by means of accurate experiments, but the reſult proved, that in none of the above-mentioned caſes the vegetation of plants appeared to be in the leaſt influenced, either by the natural or artificial electricity. He expoſed growing plants, and the ſeeds of plants, in earthen pots or

* See the Abbé Bertholon's *Electricité des Meteores.*

† See Giardini's Diſſertation *de influxu electricitatis atmoſphericæ in vegetantia,*



furniſhed

furniſhed with proper nouriſhment, to the conſtant action of electricity for a conſiderable time; and, for the ſake of compariſon, kept other plants, and other ſeeds of ſimilar plants, in the ſame ſituation, &c. but out of the influence of electricity. He found that the vegetation of the latter, did not appear to be either more or leſs forward, than that of the former. The experiments were repeated on different plants, and with various diſpoſitions of the apparatus; but the reſult was conſtantly the ſame, viz. that, whether they were electrified or not, and whether they were electrified poſitively or negatively, they grew with equal quickneſs and ſtrength, provided every other circumſtance remained unaltered *.

* "Je faiſois quelquefois ces ſortes d'experiences, en appliquant aux plantes une electricité très-foible, & d'autres fois beaucoup plus forte, ſans que j'ai jamais pu obſerver que les plantes expoſées à un degré quelconque d'electricité, ayent proſperé plus que celles qui n'étoient pas electriſées du tout. Il m'a même paru plus d'une fois, que celles qui ne l'avoient été electriſées, étoient un peu moins avancées que les autres qui ne l'avoient pas été du tout."

In the courſe of thoſe experiments, the Doctor obſerved the extenſive influence of light on vegetation, and found it to be ſo great, that he very juſtly attributes the miſtakes of former experimenters, principally, to the neglect of that circumſtance. That growing plants are in want of light, has been long known, but Dr. Ingen-Houſz found, that if adult plants wanted light, the germination of ſeeds is ſo far from being aſſiſted by that element, that it is conſiderably retarded by the preſence of it *.

With

* "Non content de ces experiences, j'en fis d'autres, infiniment plus concluantes, en ſemant des grains de moutarde & de creſſon ſur les plus grands plats de fayence que je pouvois trouver, couverts de papier brouillard, & arroſés continuellement rempli d'eau. Chacun de ces plats étoit parſemé de plus de mille graines. Je tenois les plats électriſés nuit & jour, de la maniere que M. Schwankhard à decrite dans la lettre citée à M. Ehrmann, & que je m'abſtiendrai de répéter ici, afin de ne pas groſſir inutilement le mémoire. La végétation de ces eſpèces de petites forets, étoit toujours plus ou moins précoce, à meſure du plus ou moins de lumière que les plantes recevoient, & l'electricité ne contribuoit abſolument en rien à les faire croitre plus promptement."

"J'ai

With respect to the other assertions, viz. of the vegetation being promoted by the vicinity of a vertical conductor, and of being checked by the horizontal wires, the observations must have been made in a very inaccurate manner; for, in the first place, by the very construction of a vertical conductor, such as is usually erected for the defence of buildings, the air about it, instead of being more electrified, is likely to be, in a great measure, deprived of its electricity by the conductor itself; and as for the horizontal conductor, or exploring wire, the electricity of the air is so little absorbed by them, that on account of their being insulated at both ends, I do not think that an atmospherical electrometer would be less affected within a yard distance from the

" J'ai observé que la lumière du soleil, si salutaire aux plantes adultes, est très-nuisible au developpement des semences, & à l'accroissement des plantes très-jeunes. C'est pourquoi les graines de moutarde, de cresson, & probablement de toute autre plante, se développent plutot étant placées au fond d'une chambre, que lorsqu'on les met près des fenetres, & c'est probablement faute de cette attention, qu'on a porté jusqu'à present un jugement erroné sur la cause de l'accroissement subit des plantes electrisées."

conductor, than it would at any other diſtance from it. But Dr. Ingen-Houſz, in order to remove every poſſible difficulty, had ſeveral conductors ſituated vertically near ſome plants, and had others ſtretched horizontally over other plants. They were left in that ſituation for a conſiderable time, but no particular effect enſued, for the plants near the conductors appeared as vigorous, as others of the ſame ſpecies ſituated at a great diſtance from the conductors.

Amongſt the other vegeto-electrical curioſities, it was ſaid, ſome time ago, that the motion of the ſenſitive plant (Mimoſa) was an electrical phenomenon, and that it would ſhut up its leaves, &c. when it was ſtruck with ſome conductor of electricity; but that if ſtruck with an electric, no contraction would enſue. My reader may reſt aſſured, that from my own experience, as well as from that of many other perſons, the aſſerted fact is by no means true. Whether that plant be ſtruck with a conductor, as the human hand, a piece of metal, &c. or with a non-conductor, as glaſs, ſealing-wax, and the like, the contractions take place

place without any apparent difference, provided the ſtrength of the ſtroke, and every other circumſtance, remains unaltered. Whether the plant be inſulated or not, is likewiſe immaterial. Imagining that the ſingular property of that plant might be ſomething analogous to the contractile power of animal bodies, which goes under the name of Animal Electricity, I applied different metals to the ſtock and leaves of a plant of that ſpecies, but I could not obſerve any particular phenomenon; for when the plant was touched with the metals very gently, no contraction took place, and when ſtruck with a certain degree of violence, then it would contract, whether it was touched with one metal, or two metals, or with any other body. In ſhort, it ſeems that the peculiar contractile property of the ſenſitive plant has nothing to do with electricity. It muſt not, however, be underſtood, that electric ſhocks, or ſtrong ſparks, have no effect upon it, for by the action of thoſe powers the plant is cauſed to contract its leaves very powerfully; but this does not ſhew that the contraction of the plant is an electrical phenomenon, ſince the elec-

tric ſhocks or ſparks ſtimulate the plant as much, or more, than the contact of other bodies, as they are applied in the uſual manner.

I ſhall conclude this ſection, with mentioning the very eaſy and quick manner in which the plant, called *Balſam* (Impatiens) is killed by means of electricity. The plants of that genus are not remarkably delicate; they grow eaſily, their ſtocks and branches are thick in proportion to the ſize of the plant, and they bear the inclemency of the weather tolerably well; yet a very ſmall electric ſhock ſent through the ſtem of a balſam, is ſufficient to deprive it of life. A few minutes after the paſſage of the ſhock, the plant will droop its head, the leaves and branches become flaccid, and, in ſhort, its vegetation is quite deſtroyed. I have, indeed, known ſome plants of that ſpecies which have revived after a day or two; but that effect ſeldom takes place. A ſmall Leyden phial, ſuch as may contain ſix or eight ſquare inches of coated ſurface, is ſufficient for this purpoſe; and it may even be effected by means of ſtrong ſparks from

§ the

the prime conductor of a large electrical machine.

In this experiment, neither the internal veſſels, nor any other part of the plant, appear to be injured, and indeed the ſize of the plant, and the inconſiderable ſtrength of the ſhock which is uſed, are ſuch as not to indicate the poſſibility of the veſſels being burſt, or of the vegetable organization ſuffering any other material derangement; it would, therefore, be uſeful to inveſtigate the immediate cauſe which occaſions the death of the plant.

Having ſubjected ſeveral other plants to the action of electric ſparks and ſhocks from Leyden phials, I have not found any that can be ſo materially hurt by an electric power, ſo ſmall in proportion to its ſize, as that which is ſufficient to deſtroy the vegetation of a balſam.

## EXPERIMENTS AND OBSERVATIONS

### CONCERNING THE

## EFFECTS OF ELECTRICITY

### ON METALLIC SUBSTANCES.

IN addition to the various effects of electricity on metallic bodies, which are contained in the preceding part of this treatise, I shall, in this section, collect together several other scattered materials of the same nature, as they may be of considerable assistance to those who are willing to follow the investigation of this curious and interesting subject.

In melting platina by means of electric shocks, I find a great difference in the results of the experiments, which seem principally to arise from the various states of that extraordinary

extraordinary ſubſtance. Having procured ſeveral ſpecimens of platina in grains, I ſubmitted them to the action of electric ſhocks, in the following manner: A groove was made, by means of an ivory paper-cutter, on a flat piece of wax. The depth of the groove might be about one-tenth of an inch. Two braſs wires were placed at the extremities of this groove, ſo that their ends came within half an inch of each other, and in this ſpace the grains of platina were laid one after the other, and care was taken that they might juſt touch each other; but no more grains of platina were uſed, than were neceſſary to complete the communication between the two wires. After this, the two wires were made to communicate with the inſide and outſide coatings of the charged electrical battery, and thus the ſhock was ſent through the platina. The grains of metal were by this means partially, but evidently, fuſed, ſo that ſometimes two or more of them adhered to each other.

As the grains of the metal, when promiſcuouſly taken, were ſometimes large, and at other times ſmall, one might have expected

expected that the ſmall grains would have been fuſed eaſier than the large ones, but this was by no means always the caſe; ſome of the ſmall grains were ſo refractory, as not to ſhew any marks of fuſion when examined with the microſcope, even after having ſuffered ſeveral electric ſhocks. On the other hand, ſome of the large grains were frequently agglutinated together by a moderate ſhock. Upon the whole, it ſeems that the whiter grains are fuſed more eaſily than thoſe of a dark grey colour.

When the grains were heaped together in the groove without any diſtinction, and as ſtrong a ſhock as the battery could afford was ſent through, the marks of fuſion were more evident than in the other caſe; but this effect ſeemed to depend on the ſmall particles of gold and of iron that were intermixed with the platina; for when the experiment was tried with ſpecimens of platina that were more pure and free from heterogeneous particles, the marks of fuſion were leſs manifeſt. After this obſervation one would naturally imagine, that the purer the platina is, the more difficult it will be

to

to fuſe it by means of electric ſhocks; but I find that filings of malleable platina, treated in the above-mentioned manner, are more eaſily fuſed than the natural grains. This platina had been rendered malleable at Paris a few years ago by a Mr. Janeti, and it was ſold as very pure platina, unmixed with any other metal.

The degree of fuſibility of different metallic ſubſtances, when expoſed to the action of electric ſhocks, is by no means the ſame as that which takes place in chemical furnaces, which ſhews that the electric fluid is not the ſame thing as the element of fire; the effects, in general, ſeeming to be proportionate to the degree of reſiſtance which it meets with in its paſſage: but with reſpect to metals, the degree of fuſibility which they ſhew in this way, ſeems to be in the compound proportion of that reſiſtance which each particular metal offers to the paſſage of the electric fluid, and of its natural degree of fuſibility when expoſed to a common fire.

Many

Many persons have attempted to determine the degree of fusibility of metals by the action of electricity; but no one seems to have succeeded so well as Mr. Van Marum, who, for this purpose, employed the famous electrical machine belonging to the Museum of Teyler (which has been described in the second volume of this treatise) and an electrical battery, which contained 225 square feet of coated surface. Mr. Van Marum had wires of different metals drawn of the same diameter, which was equal to one thirty-second of an inch, and by exposing equal lengths of them successively to the above-mentioned battery, which was charged equally high in every experiment, found, that of the leaden wire 120 inches were melted, of tin wire the like quantity, of iron wire five inches were fused, of gold wire three inches and a half, and of the silver wire, or brass, or copper, a quarter of an inch only was melted, which shews a pretty good estimate of their fusibility by the action of electricity *.

From

* The degrees of fusibility of various metallic substances when exposed to a chemical fire, or rather of the degrees

From those and some other experiments made with the same apparatus, Mr. Van Marum deduces the following conclusions, viz. that lead is the worst, and, upon the whole, copper is the most eligible metal for the construction of a conductor of lightning.

That he could not possibly determine the proportion between the lengths and dia-

degrees of heat, which are necessary to fuse them, as calculated by the Academicians of Dijon, are as follow:

| | |
|---|---|
| Tin - - | 170° of Rheaumur's thermometrical scale. |
| Lead - - | 230. |
| Silver - | 450. |
| Gold - - | 563. |
| Copper - | 630. |
| Iron - - | 696. |

According to the experiments of the late Mr. Wedgwood, F. R. S. the degrees of heat, which are necessary to melt metallic bodies, are as follow:

| | |
|---|---|
| Brass - - - | 3,807° of Fahrenheit's thermometrical scale. |
| Swedish copper - | 4,587. |
| Fine silver - - | 4,717. |
| Fine gold - - | 5,237. |
| Cast iron - - | 17,977. |

See the Phil. Transf. Vol. LXXII.

meters

meters of metallic wires, that could be melted by the power of the Teylerian machine.

That iron, tin, and copper, were melted into globules, but this was not the caſe with the other metals.

That the metallic globules were ſometimes thrown to the diſtance of thirty feet, and upwards.

That the globules of tin remained red hot for about eight or ten ſeconds; and

That when the wires were very long, the fuſion was but partial.

Mr. Van Marum had the curioſity of trying whether the metallic wires could be fuſed and calcined in water, viz. by ſending the charge of the battery through them whilſt ſtanding under water, and he ſucceeded whenever he uſed the eighth part of that length of wire, which would have been calcined if the experiment had been performed in air.

The

The eaſineſs with which iron and ſteel are fuſed by means of electricity, and the violence with which ſmall particles of them burn, renders them peculiarly uſeful in entertaining experiments. The filaments produced in turning of ſteel are ſo very readily ignited by an electric ſhock, that ſuch of them as are very fine, may be burned by means of a ſix-ounce coated phial, or even a ſmaller one. They may be alſo burned by a ſingle ſpark from a large prime conductor. If one of thoſe filaments or ſhavings, which may be frequently procured at the opticians ſhops, be put through a cartridge, the gun-powder may, in that caſe, be fired by means of a very ſmall electric ſhock.

## NOTES AND ADDITIONS

### TO VARIOUS PARTS OF THIS TREATISE.

### N° I.

AN experiment is defcribed in page 324 of Vol. I. which fhews that hot air is a conductor of electricity. From the manner in which that experiment is performed, one may be led to fufpect that the effluvia which proceed from red-hot iron may contribute towards rendering the air a conductor. It appears likewife, that the air muft be heated to a confiderable degree in order to become a conductor. Without entering at prefent into a minute difcuffion of thofe particulars, I fhall here only fubjoin an experiment of Mr. READ, by which he means to prove that hot air is not a conductor.

"IT

" IT has been alſo commonly ſaid " that hot air conducts electricity. With " a view to aſcertain this matter, the fol- " lowing experiments were made: To one " end of a long piece of wood (which ſerv- " ed as a handle) was fixed a glaſs rod fif- " teen inches long; to the remote end of " the glaſs was fixed a pith-ball electrome- " ter. Having electrified the balls, I held " them by the wood handle, and projected " them into a large oven, immediately after " the fire was drawn out of it; the conſe- " quence was, that when I performed the " operation ſlowly, the balls loſt their elec- " tricity; but that when done quick, with " as little delay as poſſible, their electric " charge was not diminiſhed. The loſs of " electricity, in the firſt caſe, was found to " have eſcaped along the glaſs into the " wooden handle, and ſo to the earth, ow- " ing to the great heat the glaſs rod had " acquired, by which it became a conductor " of the fluid, for until it had cooled a lit- " tle the balls could not be charged again.

" I ſhall lay before the reader one cir- " cumſtance more, becauſe it may tend to

" throw light on what degree of heat the " oven was in at the time the obſervations " were made. The baker having pointed " out to me the hotteſt part of the oven; " with a quick motion in and out, I plung-" ed the electrified balls into that part of " it, by which one thread and ball was " burned off, but the remaining ball ſhew-" ed that it ſtill retained its electric charge, " becauſe it was ſtrongly attracted on the " approach of my finger *."

The focus of a burning lens or mirrour is not a conductor of electricity. Let a wire that proceeds from the outſide of a charged Leyden phial, come within an inch of the knob of the jar, or, in ſhort, ſo as to be very little farther from it than the ſtriking diſtance. Let then the focus of the ſolar rays that are collected by a lens or mirrour, fall midway between the knob of the phial and the wire which proceeds from its outſide, and it will be found, that the charge of the phial is not thereby diſſipated; whereas, if

* Read's Summary View of Spontaneous Electricity, p. 8 and 9.

the

the flame of a candle, or any other conductor of electricity be interposed between the knob and the wire, the discharge will immediately take place. This experiment seems to corroborate the supposition, that the rays of the sun, or of light in general, have no heat in themselves, but only extricate the elementary heat from such bodies as they happen to fall upon, provided those bodies obstruct their course, and are not transparent.

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## N° II.

*The following paragraphs contain additional properties of charged electrics.*

"IT is unnecessary to insist upon what is called the equilibrium of an electrical charge, because Dr. FRANKLIN has admirably explained it according to his hypothesis. But there is another important particular, which has been almost entirely overlooked, namely, the uncompensated electricity, which is as essential to the charge

as that which is in equilibrio. Whenever a jar is charged, the greateſt part of the electricity becomes latent on account of the compenſation; but there is a certain proportion which remains on the inſulated ſide, and exerts its force to prevent the electricity from returning to the outer ſurface. In moderate intenſities this will explode, and carry the charge with it to diſtances which are in proportion to the quantity of the charge itſelf; but in greater intenſities the diſtances greatly exceed that proportion. With glaſſes of different thickneſſes, this intenſity, as meaſured by the exploſive ſpark, is as the thickneſs when the charges are equal, as Mr. Cavendiſh has determined, and I find likewiſe by experiments with thin ſubſtances; but when the thickneſſes are greater, it increaſes in a higher proportion, as is found by the exploſion which takes place between the electrophore and its plate, as well as by other experiments.

“ This uncompenſated part of the charge (which is commonly in proportion to the quantity of latent or compenſated electricity, or to the diſtance at which it exerts its

action)

action) was found to be greatly increafed when a feries of jars were made to charge each other. If a jar be infulated, and made to explode by LANE's electrometer at a determinate number of turns, and another jar be then connected wirh its external coating, fo as to become charged by that means, the explofion from the outfide of the laft, to the infide of the firft, will take place at the electrometer (unaltered) with much fewer turns. Or if the electrometer be altered till the explofion takes place at the original number, the diftance will be much greater than before. Hence we fee, that the intenfity of the uncompenfated part muft be greater when there is a greater charge to be maintained, whether it be on one furface only, or on two furfaces fucceffively connected.

"It is evident, that the breaking of jars is not effected by any attraction between the electricities which form the charge, but by this neceffary furplus; for thicker glaffes require much lefs electricity to produce an intenfity which breaks them, than thinner do; and I found a piece of Mufcovy talc,

one-hundredth of an inch thick, to bear a charge consisting of ten times the quantity of electricity, which was sufficient to have charged an equal surface of common glass, so as to break it; but the intensity of the very dense charge on the talc was so low as to afford an explosion of no more than about one-tenth of an inch, while that of the glass jar it was compared with exploded through about five inches.

"The perforation of glass by the long spark, or by the spark through oil or cement, seems to depend on the very great intensity of the electricity which has not time to diffuse itself, but charges a minute part of the surface very high.

"Muscovy talc being a very perfect non-conductor, and capable of being divided into plates of less thickness than one two-hundredth part of an inch; I made many experiments with it, which are too numerous to enter into this paper. In consequence of its great capacity it gives very strong shocks. Contrary to the assertion of BECCARIA, I found that its laminæ are naturally

turally in ſtrong oppoſite ſtates of electricity, and flaſh to each other when torn aſunder in the dark. A large piece being ſplit in two, the parts were found to be in oppoſite ſtates. The greateſt care was taken in theſe experiments to avoid friction, and to uſe ſuch pieces as had never been excited, nor brought near the machine *."

It is well known, that after the diſcharge of the Leyden phial, a reſiduum of electricity always remains on the glaſs whether coated or not. This reſiduum is very ſeldom to be perceived immediately after the diſcharge; but it becomes manifeſt a certain time after, continues increaſing to a certain degree, and then decreaſes until it entirely vaniſhes. Mr. Read has obſerved, that the accumulation of this reſiduum is accelerated by heat. "I was," *ſays he*, "ſhewing a gentleman the power and utility of Mr. VOLTA's condenſer of electricity; in order to which, a Leyden bottle was charged, and then diſcharged, by touching

* Mr. Nicholſon's Experiments and Obſervations on Electricity.—Phil. Tranſ. Vol. LXXIX.

its coated ſides with a diſcharging rod. It now appeared to be deprived of all its electricity; yet if I held it by the coating, and touched with the knob of it the metal plate of the condenſer (placed upon an imperfect conductor) and held them in contact a few ſeconds of time, then removed the bottle, and took up the metal plate by its inſulating handle, and preſented it to a ſenſible electrometer, the electricity was ſtrong enough to enable me to aſcertain its quality. But theſe beautiful effects diminiſhed by repetition; and I repeated the experiment until no electric ſigns appeared in the condenſer. However, while we were talking the matter over, I ſtill held the bottle in my hand, and after a long interval of time, from our laſt trial, I repeated the operation once more; and, to our aſtoniſhment, we found the bottle ſtill ſtrongly electrical. This new revival of the electricity we could attribute to nothing but the diffuſive warmth of my hand, acting on the electricity of the bottle. This ſet us to work again, and to try the bottle by turns in a warm and cold ſtate, and we found that our conjecture of the

the cause was confirmed by repeated trials *."

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## N° III.

THE amalgam of zinc and quicksilver, described in page 142 of vol. I. is apt to become very hard, especially when left untouched for some time; this may be partly prevented by mixing it with a little grease, as tallow, or mutton-suet, and a very small portion of finely powdered whitening. The proper way of preparing this amalgam is the following: Let the quicksilver be heated to about the degree of boiling water, and let the zinc be melted in a crucible or iron ladle. Pour the heated quicksilver into a wooden box, and immediately after pour the melted zinc in it. Then shut the box, and shake it for about half a minute. After this you must wait until the analgam is quite cold, or nearly

* Read's Summary View of the Spontaneous Electricity, p. 16.

so,

ſo, and then you may mix the greaſe with it by trituration.—If the melted zinc be poured into the quickſilver when cold, a very ſmall portion of the former will be amalgamed, the reſt remaining in lumps of different ſizes.

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## N° IV.

*Of the ſuſpected ſpontaneous production of Electricity from living animals.*

SEVERAL perſons have ſuſpected, and ſome have even aſſerted their having diſcovered, that living animals generally yield ſome electricity, which, though moſtly poſitive, is however ſometimes negative. Thoſe ſuſpicions and aſſertions, joined to the recent diſcovery of that property of animal bodies which has been called *animal electricity*, induced me to try whether any ſuch ſpontaneous production of electricity could be aſcertained or diſproved; and with this view I made ſome experiments, the reſult of which ſeemed to ſhew, that no electricity

was

was produced by any natural proceſs in the animal œconomy. The method which I adopted was as follows: I inſulated either myſelf or ſome other perſon, and after a certain time I examined, by means of an electrometer, whether any electricity was produced. The electrometer was generally electrified, in a ſmall degree, either poſitively or negatively; but it ſeemed to me that this electricity was either produced from ſome accidental friction, or it was communicated by the ſurrounding air; for when the electricity of the atmoſphere was very weak, and the inſulated perſon avoided every ſort of movement as much as it was poſſible, the electrometer ſhewed no ſigns of electricity.

In thoſe experiments I did not think proper to uſe the multiplier, conſidering, that as a ſmall quantity of electricity generally adheres to a perſon's cloths, to the hair, &c. one might be miſled by it, ſo as to imagine that this electricity was produced by the natural functions, or reſpiration, of the animal body itſelf.

N° V.

## N° V.

*Obſervations and experiments relative to the Electricity produced by evaporation.*—See page 267 and 268 of vol. II.

MR. VOLTA's diſcovery of the electricity which is obtained from the evaporation of water, and ſuch other ſolid or fluid bodies as can be reduced into ſmoke or vapour; beſides its affording an eaſy explanation of the origin of the electricity in the clouds, &c. ſeemed to point out a general law of nature, viz. that the capacity of water, and other fluids, for holding the electric fluid, was increaſed by thoſe bodies being expanded into the ſtate of elaſtic vapour, and was diminiſhed by thoſe bodies being condenſed into their other fluid form. Therefore, in the firſt caſe, by imbibing an additional quantity of electric fluid, they would leave the bodies, that happened to be in contact with them, in a negative ſtate; and, in the ſecond caſe, by giving out a quantity of electric fluid, they would leave the bodies, that happened to be in contact with them, in a poſitive ſtate.

All

All the experiments made on evaporation for some years after this discovery, were attended with results conformable to the above-mentioned general law; but two remarkable exceptions have of late been discovered, which, besides their contradicting the said law, seem to point out a more intimate connection between the electric fluid and other bodies. The first of those exceptions was discovered and published three years ago, by a learned professor of the Academy of Mantua; the second was very lately discovered by myself.

The Mantuan Professor observed, that when water was evaporated by being put in contact with a red-hot piece of rusty iron, it would leave the iron electrified positively; whereas when the experiment was tried with a clean piece of iron, the electricity acquired by the metal would be of the negative kind *.

When

* Parvam et tenuem laminam ferream, quatuor aut quinque pollicum extensam, et ferrugine undique tectam, data opera selegi, hanc intra carbones tandiu detinui, quandiu candens fieret caute agens ne rubigo perderetur,

When I first attempted to repeat this curious experiment, the result did by no means answer my expectations; the electricity, which was produced being of the negative, and not of the positive kind; but observing that sometimes no sensible degree of

tur, hanc projeci intra vas æneum tanta aqua plenum, quanta sufficeret ad totam laminam tegendam; ex extinctione lamellæ ebullitio, et evaporatio maxima orta est, et investigata electricitas subito deprehensa est positiva ad lineas duas, et ultra, quæ paulatim minuebatur, prout minuebatur calor, ebullitio, et evaporatio, et istis cessantibus cessavit et ipsa. Alio frustulo ferreo simili similiterque ferrugineo, et fortasse a primo separato, et secto expertus sum, et positivam etiam obtinui duarum linearum. Præterea iisdem frustulis iterum usus sum, et nullam amplius habui positivam electricitatem, amiserunt ipsa omnem ferruginem suam a primis experimentis; tentavi per alia frustula ferruginea, æque positivam dederunt; potuit ne aliquid præstare data ferrugo, quæ maxima erat, et ubique per universam superficiem dispersa ad reddendam electricitatem positivam, quæ cæteroquin negativa esse debebat ob nimiam et repentinam evaporationem. Josephi Gardinii Dissertatio de Electrici Ignis Natura, p. 124.

The same gentleman observed, that in other experiments of the same nature, the positive electricity was also produced; but that effect was not constant, nor have I been able to obtain the same result.—See the above quoted work, p. 126, 129, 133.

electricity

electricity was produced, though the evaporation was very quick and copious, I began to suspect that the iron, which I had employed, was not sufficiently covered with rust, in consequence of which two opposite states of electricity might possibly be produced, viz. the negative from the clean, and the positive from the rusty, part of the iron: which two opposite states, by counteracting each other, would leave the iron un-electrified. After various repetitions of this experiment, in which either the red-hot iron was thrown into the insulated water, or the water was poured upon the red-hot and insulated iron, I found that this was actually the case.

I procured some old iron nails, which had remained exposed to the atmosphere for several years, and of course had contracted a very thick coat of rust; and on performing the experiment with them, I obtained positive electricity, agreeably to the assertion of the above-mentioned gentleman. The same nail very seldom would answer for more than one experiment; for the action of the fire and of the water generally removed a great

deal of the ruſt, and exhibited the naked metal, which would afterwards acquire the negative electricity. Here follows the manner of performing this remarkable experiment.

Inſulate a metallic or earthen plate, and pour a ſmall quantity of water in it, and let a ſenſible electrometer be connected with the water; then drop a red-hot piece of iron into the plate, and it will be found, that, if very ruſty iron be uſed, the electrometer will be opened with poſitive electricity; if the iron be clean, or free from ruſt, the electrometer will acquire the negative electricity; and laſtly, if the iron be partially ruſty, the electrometer will acquire little or no electricity, though in every caſe the evaporation may be equally quick and copious.

The other exception of the above-mentioned general law is ſhewn by means of red-hot glaſs, which I chanced to diſcover very lately. The various degrees of electric power that are produced by the evaporation of water from different ſubſtances induced me

me to diverſify the experiments as much as I could, in order to diſcover, if poſſible, the reaſon why thoſe different effects took place when the evaporation ſeemed to be equally quick and copious. Amongſt other ſubſtances, I tried glaſs, and found that it generally produced little or no electricity. The water was ſometimes poured upon the hot glaſs, but in general the hot glaſs was dropped into the inſulated water, which was contained in a tin cup. However, the difference of effect was found not to be occaſioned by thoſe two different modes of proceeding. Having repeated this experiment a great many times, I at laſt found, that the effect depended on the different nature of the glaſs. If white and clean flint glaſs be made red-hot, and in that ſtate be dropped into the veſſel of water, a quick evaporation will enſue, and the veſſel is electrified poſitively. If the flint glaſs be not very clear, there will not be any electricity generated by the evaporation, &c. And laſtly, if the experiment be tried with more impure glaſs, as the glaſs of which wine bottles are made, the negative electricity will be produced.

In performing this experiment, it is neceſſary to take care that no pieces of coal adhere to the glaſs, which will frequently happen when a piece of glaſs is heated in a common fire; for in that caſe negative electricity will be produced by the evaporation, though the beſt flint glaſs be uſed.

It has frequently happened, in the courſe of my experiments, that no electricity whatever has been produced by the evaporation of water from certain ſubſtances; however, as in thoſe caſes the evaporation was not very copious, I attributed the deficiency of electricity to the weakneſs of the evaporation. But a very remarkable inſtance of this ſort is mentioned in the diſſertation of the above-mentioned ingenious Profeſſor *. He ſlaked 25 pounds weight of quicklime with a ſufficient quantity of water, and though a very copious evaporation took place, yet it was not attended with any electricity. Should any perſon ſuſpect, that the deficiency of electricity in this experiment was owing to the want of burning coals or

* Page 131.

actual fire, he ſhould conſider, that in other ſimilar proceſſes electricity is produced without any actual fire; thus the evaporation, which is occaſioned by the effervеſcence of iron filings in diluted vitriolic acid, produces negative electricity.

After a careful examination of the abovementioned experiments, the origin of the electricity, which is obſerved in the evaporation of water and other evaporable ſubſtances, whether ſolid or fluid, ſeems not to be reconcileable to the general law already noticed, nor can I form any plauſible theory that can be ſufficient to account for all the phenomena. If the production of electricity in thoſe experiments depended upon the increaſed or diminiſhed capacity of water for holding the electric fluid, it ſhould ſeem to be immaterial whether the water be evaporated in one way or in another, provided the evaporation be made with equal quickneſs and in equal quantities. Were it not known that glaſs or iron made red-hot produces no electricity in cooling, we might ſuſpect, that the electricity, which is produced by the evaporation of water, may be

counteracted by the contrary electricity, which is produced by the cooling of glaſs or iron; but it has been obſerved by ſeveral ingenious perſons, that red-hot glaſs and red-hot iron produce no electricity whatever when ſuffered to cool upon inſulated ſtands.

Conſidering the circumſtances which might be common to white flint glaſs and to calcined iron, it occurred to me, that as they were both deſtitute of the power of inflammability, or of phlogiſton*, therefore other ſubſtances, that were in the ſame predicament, might produce the like effect, viz. might acquire the poſitive electricity from the evaporation of water thrown upon them after being made very hot. With this view I tried a piece of a tobacco-pipe made hot; I alſo tried another piece of earthen-ware of a very refractory nature,

* Having not mentioned any of the new chemical names in the courſe of this work, it will be uſeleſs in this place to take any notice of the antiphlogiſtic theory; but it will be ſufficient to obſerve, that the remark concerning the nature of diverſe ſubſtances, &c. is applicable to either theory.

and

and likewiſe tried flints; but the evaporation of water thrown upon thoſe ſubſtances always produced the negative electricity. The elucidation therefore of thoſe phenomena muſt be deferred until other facts are aſcertained by future experiments. For the preſent we can only ſay, that the electricity, which is manifeſted in the evaporation, ſeems to depend on ſome other circumſtance beſides the mere expanſion of water or other fluids into the form of elaſtic vapour.

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## N° VI.

### *Deſcription of the ſelf-charging Leyden phial.*

TAKE a glaſs tube of about eighteen inches in length, and an inch, or an inch and a half, in diameter. It is immaterial whether one of its ends be cloſed or not. Coat the inſide of it with tin-foil, but only from one open extremity of it to about as far as its middle; the other part, which re-

mains uncoated, we ſhall call the naked part of the inſtrument. Put a cork to the aperture of the coated end, and let a knobbed wire paſs through the cork, and come in contact with the coating. The inſtrument being thus prepared, hold it in one hand by the naked part, and with the other hand clean and dry-rub the outſide of the coated part of the tube; but after every three or four ſtrokes you muſt remove the rubbing hand, and muſt touch the knob of the wire, and in ſo doing a little ſpark will be drawn from it. By this means the coated end of the tube will gradually acquire a charge, which may be increaſed to a conſiderable degree. If then you graſp the outſide of the coated end of the tube with one hand, and touch the knob of the wire with the other hand, you will obtain a ſhock, &c.

In this experiment the coated part of the tube anſwers the double office of electrical machine and of Leyden phial; the naked part of it being only a ſort of handle to hold the inſtrument by. The friction on the outſide of the tube accumulates a quantity

quantity of poſitive electricity upon it, and this electricity, in virtue of its ſphere of action already explained in the firſt volume, forces out of the inſide a quantity of electricity alſo poſitive. Then by taking the ſpark from the knob, this inſide electricity, which is by the coating communicated to the knob through the wire, is removed, conſequently the inſide remains undercharged or negative, and of courſe the poſitive electricity of the outſide comes cloſer to the ſurface of the glaſs, and begins to form the charge. By farther rubbing and taking the ſpark from the knob this charge is increaſed, &c.

Inſtead of a tube this inſtrument may be conſtructed with a pane of glaſs, in which caſe it will be rather ſimpler, but it cannot be managed ſo eaſily, nor of courſe can it be charged ſo high as the tube. A piece of tin-foil muſt be paſted in the middle of only one ſurface of the pane, leaving about two inches and a half or three inches of uncoated glaſs all round. This done, hold the glaſs by a corner, with the coated ſide from you, and with the other hand rub its uncoated

uncoated ſide, and take the ſpark from the tin-foil alternately, until you think that the glaſs may be ſufficiently charged; then lay the glaſs with its uncoated ſide flat upon one open hand, and on touching the tin-foil with the other hand you will receive the ſhock.

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## N° VII.

*Of the decreaſe of the denſity of electricity in electrified atmoſpheres.*

FROM the analogy of other emanations that proceed from a centre, and are expanded in all directions, it might have naturally been concluded, that the intenſities of electric atmoſpheres (by which we only mean the power of acting on other bodies) at various diſtances from the electrified bodies, are inverſely as the ſquares of thoſe diſtances. Thus heat, light, and the force of gravity decreaſe as the ſquares of thoſe diſtances, or, which is the ſame thing, their intenſities, are inverſely as the ſquares of the diſtances from the centres from which

which they emanate; ſo that at three times a certain diſtance they are nine times weaker, at five times that ſame diſtance they are twenty-five times weaker, and ſo on *. But independent of this analogy, it has been proved by actual experiments, and by mathematical reaſoning, which was deduced from thoſe experiments, that ſuch is the law which is followed by the intenſities of electric atmoſpheres.

It has been ſhewn in the courſe of this work, that when an inſulated conducting

* The uſual way of ſhewing the truth of this general law, is by demonſtrating, that the ſurfaces of ſpheres are as the ſquares of their diameters, or the ſquares of their radii; for if we conceive two ſpherical ſurfaces to be deſcribed at two given diſtances from a luminous centre, for inſtance, at the diſtances of one and two feet, it is evident, 1ſt, that the ſame rays of light which paſs through the one muſt likewiſe paſs through the other of thoſe ſpheres; 2dly, that as the ſecond ſpherical ſurface is larger than the firſt, the rays muſt be proportionably more expanded at the ſecond ſpherical ſurface than at the firſt; but the ſpherical ſurfaces are as the ſquares of the diſtances, which ſquares are one and four; therefore the rays of light, or of any other of the above-mentioned emanations, muſt be four times more expanded or leſs intenſe at two feet than they are at one foot diſtance from the centre.

body

body is brought within the ſphere of action of an electrified body, the former acquires the two electricities on oppoſite ſides: and that in ſome place called the *neutral point*, which lies between the two contrary electrified extremities, the inſulated body remains in its natural ſtate. Now Earl Stanhope firſt determined mathematically the ſituation of ſuch neutral point in a cylindrical body properly ſituated, upon the ſuppoſition that the intenſity of electric atmoſpheres decreaſed in the ſimple proportion; and likewiſe upon the ſuppoſition of its decreaſing in the duplicate proportion (viz. as the ſquare) of the diſtance. Then he proceeded to experiments, and by preſenting an electrometer to various parts of the inſulated cylindrical body, found that the place which neither attracted nor repelled the electrometer coincided with the neutral point which had been determined upon the ſuppoſition that the intenſity of electric atmoſpheres decreaſed as the ſquare of the diſtance *.

* See Lord Mahon's (which was Earl Stanhope's former title) Principles of Electricity, Part IV. V. and VI.

The

The ſame law of electric atmoſpheres has been alſo confirmed experimentally by other perſons. Their method in general has been to examine, by means of ſcales and weights, or of ſimilar inſtruments, the actual attractive force exerted towards a certain body by an electrified body placed at different diſtances.

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## N° VIII.

*Farther uſes to which the electric power may be applied, in addition to chap. the 9th of part I.*

THE attempts recently made to convey intelligence from one place to another at a great diſtance, with the utmoſt quickneſs, have induced me to publiſh the following experiments, which I made ſome years ago, and of which I ſhould not have taken any farther notice, had it not been for the above-mentioned circumſtance, which ſhewed that they may probably be of uſe for that and other purpoſes.

The

The object for which thofe experiments were performed, was to fire gun-powder, or other combuftible matter, from a great diftance, by means of electricity. At firft I made a circuit with a very long brafs wire, the two ends of which returned to the fame place, whilft the middle of the wire ftood at a great diftance. In this middle an interruption was made, in which a cartridge of gunpowder mixed with fteel filings was placed. Then, by applying a charged Leyden phial to the two extremities of the wire, (viz. by touching one wire with the knob of the phial, whilft the other was connected with the outfide coating) the cartridge was fired. In this manner I could fire gun-powder from the diftance of three hundred feet and upwards. But this method was fubject to the following inconveniencies. As the wire was laid upon the ground, the experiment would anfwer only when the ground was very dry. It was found neceffary to ufe a large jar for the purpofe, viz. one which contained about three fquare feet and a half of coated furface; and laftly, it proved very troublefome to difpofe the two wires between the cartridge and the

Leyden jar ſo as not to touch each other in ſome intermediate place; for when that happened the powder could not be fired, the charge naturally paſſing from one wire to the other at the place of their mutual contact.

In order to avoid the laſt-mentioned inconvenience, I tried the experiment with one wire. A braſs wire of about a 50th part of an inch in diameter, and 200 feet long, was laid on the ground, and its extremity was inſerted in the cartridge of gun-powder and ſteel filings. Another piece of the ſame ſort of wire was likewiſe inſerted with one end into the ſame cartridge, and its other end was thruſt into the ground. Then, by applying the knob of the charged jar to that extremity of the long wire which was remote from the cartridge, whilſt the outſide of the jar communicated with the ground, I expected that the circuit would be formed partly through the wire and partly through the ground, and of courſe that the powder would be fired as in the preceding experiment. That the charge of the jar paſſed through the wire, and through

through the ground, was evidently proved by the powder being ſometimes fired; but part of the charge was certainly diſſipated before it reached the cartridge; for in this diſpoſition of the apparatus a much greater charge was required to produce the deſired effect, ſo that a battery of nine ordinary cylindrical jars would but juſt ſuffice when the ground was dry, and every other circumſtance was favourable.

Conſidering that the management of ſuch a battery as might be ſufficient for this purpoſe could not be charged or moved about very eaſily, I attempted to uſe ſome more inflammable ſubſtance inſtead of gun-powder. Phoſphorus was the ſubſtance which I tried next. The apparatus was diſpoſed as in the laſt experiment, excepting that in the place of the cartridge a ſmall piece of phoſphorus was ſituated, into which the two extremities of the wires were thruſt, ſo as to come within about a fiftieth part of an inch from each other.—In this caſe a ſmaller charge even than that which had been uſed in the firſt experiment was found ſufficient to inflame the phoſphorus; yet this method

was found ſubject to other inconveniences. Sometimes the phoſphorus would be ſplit without taking fire; at other times the charge would not paſs through it; at leaſt it ſhewed no ſigns of ſuch paſſage, every thing remaining unaltered. It was alſo found neceſſary to try the experiment ſoon after ſituating the phoſphorus in its proper place; for if three or more hours elapſed, either the ſoftening or waſting of the phoſphorus prevented the ſucceſs.

I endeavoured to ſubſtitute other combuſtible ſubſtances; but after all I found none to anſwer either better or ſo well as inflammable air. After having diſcovered that this permanently elaſtic fluid was by far the readieſt to be inflamed by means of electricity, I tried to improve the wire of communication, and after ſeveral attempts I at laſt conſtructed one, which might be laid upon dry or wet ground, and even partly through water.

Without detaining my reader any longer with the narration of the intermediate trials, I ſhall ſubjoin the deſcription of the me-

thod which I found to ſucceed beſt, and by which means gun-powder, or other combuſtibles, may be certainly fired from the diſtance of two or three hundred feet, and probably from a much greater diſtance without danger to the operator, and without any loſs of time; for the inflammation will take place at the moment that the knob of the charged jar is preſented to the extremity of the long wire.

The inflammable air muſt be contained in a two or three ounce phial, ſuch as is uſed for medicines, which muſt be prepared in the following manner: Turn the phial with the bottom upwards, and by ſtriking the apex of the conical cavity with a pointed thick wire, a hole will be made in the bottom of the phial. In this hole a piece of thin wire muſt be cemented, ſo as to project about an inch within the phial, and its external part muſt be bent in the form of a hook. A very ſound cork muſt be fitted to the mouth of the phial, and another piece of thin wire muſt be paſſed through it, ſo as to project within the phial, and to come with its extremity to the diſtance of about

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the fortieth part of an inch from the extremity of the wire that proceeds from the bottom. The external part of this wire in the cork ſhould be about two or three feet in length. The phial being thus prepared, remove the cork from it, and place it inverted for about four or five ſeconds of time, over the mouth of another large phial full of inflammable air; then ſlip it off, and cork it up as faſt as you can. By this means a certain quantity of inflammable air will be introduced, which, by mixing with the common air contained in the phial, will form a compound elaſtic fluid, which is ſo ready to take fire, that if the leaſt electric ſpark be paſſed from wire to wire through the phial, the air will inſtantly explode, and the cork will be puſhed out of it with violence.—The long wire of communication is prepared in the following manner:

A piece of annealed copper or braſs wire, of about a fiftieth or fortieth part of an inch in diameter, being ſtretched from one ſide of a room to the other, heat it ſucceſſively from one end to the other by means of the flame of a candle, or of a red-hot

piece of iron, and as you proceed with the candle, rub a lump of pitch over the heated part of the wire. When the wire has been thus covered with pitch, a ſlip of linen rag muſt be put round it, which can be eaſily made to adhere to the pitch, and over this rag another coat of pitch muſt be laid with a bruſh, the pitch being melted in a pipkin or other convenient veſſel. This ſecond layer of pitch muſt be covered with a ſlip of woollen cloth, which muſt be faſtened by means of a needle and thread. Laſtly, the cloth muſt be covered with a thick coat of oil paint, and when the paint is dry, the covered wire may be uſed for the experiment. In this manner many pieces of wire, each of about twenty or thirty feet in length, may be prepared, which may afterwards be joined together, ſo as to form one continued metallic communication; but care muſt be taken to ſecure the places where the pieces are joined, which is moſt readily done by wrapping a piece of oil-ſilk over the painted cloth, round the two contiguous extremities, and binding it with thread. When a long wire has been thus made out of the various ſhort pieces, let one end of it, pur-

poſely

posely left out beyond the above-mentioned envelopes, be formed into a ring, and to the other extremity adapt a small brass ball.

In order to perform the experiment, lay the wire upon the ground in any direction, fasten the hooked wire of the phial that contains the inflammable air to the ring at one end of it, and push the extremity of the wire that comes out of the cork of the said phial about an inch or two into the ground. Then, if you bring the knob of a charged Leyden phial in contact with the ball at the other extremity of the long wire, the inflammable air will explode immediately, and the cork will be pushed out of the neck of the phial.

For this purpose a Leyden phial, that contains about one square foot of coated surface, will be found to be quite sufficient, when the distance between the operator and the spot in which the inflammation is to take place does not exceed 200 feet. For greater distances it will be necessary to use Leyden jars of proportionably larger size.

Thus we have ſhewn how inflammable air may be fired; but in order to ſet fire to gun-powder, the only addition which needs be made, is to ſurround the cork and neck of the phial with looſe cotton, that has been previouſly filled with pounded gun-powder, from which a quick match may be continued, &c.; for when the cork is puſhed out of the neck of the phial by the exploſion of the inflammable air, a flame comes out which has power ſufficient to ſet fire to the gun-powder in the cotton. I have ſometimes put a ſmall quantity of cotton rubbed in gun-powder into the phial of inflammable air, beſides the cotton on the outſide; but it is by no means neceſſary.

The only inconvenience which I have found to attend the above-deſcribed method is, that ſometimes the very nature of the inflammable air in the phial ſuffers ſuch an alteration as to looſe its inflammability, in which caſe the experiment will not be attended with the deſired effect. But this degeneration of the inflammable air does ſeldom, if ever, take place in leſs than two or three days, and ſometimes not even in a fortnight;

fortnight; ſo that one may be ſure that the experiment will anſwer within 24 or 30 hours. And it is probable that it will anſwer within two or three days. It is immaterial whether the phial be left attached to the long wire on the ground all that time, or be faſtened to it the moment before the time of the inflammation.

If this experiment be tried without any inflammable air in the phial, the only effect will be, that a ſpark will be ſeen between the two wires in the phial at the moment that the charged jar is applied to the knobbed end of the long wire. By ſending a number of ſparks at different intervals of time, according to a ſettled plan, any ſort of intelligence might be conveyed inſtantaneouſly from the place in which the operator ſtands to the other place in which the phial is ſituated. With reſpect to the greateſt diſtance to which ſuch communication might be extended, I can only ſay, that I never tried the experiment with a wire of communication longer than about 250 feet; but from the reſults of thoſe experi-

ments, and from the analogy of other facts, I am led to believe that the abovementioned ſort of communication might be extended to two or three miles, and probably to a much greater diſtance.

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## N° IX.

### *Preparation of the Electrical Amber Varniſh.*

IT is well known that glaſs frequently becomes a conductor of electricity, in conſequence of its being apt to attract moiſture from the atmoſphere; to avoid this inconvenience, the makers of philoſophical inſtruments generally cover the glaſs legs and other parts of the electrical apparatus with a coat of ſome non-electric ſubſtance. Sealing-wax and amber varniſh have been principally uſed for this purpoſe. The ſealing-wax has been laid on the glaſs two different ways, viz. either by making the glaſs ſufficiently hot, and then rubbing a piece of ſealing wax upon it, or by firſt diſſolving the ſealing-wax in ſpirit of wine, and then lay-

ing it upon the glaſs with a hair pencil*. The firſt of thoſe methods is in my opinion the beſt of any, but it is not practicable with large pieces of glaſs; for, beſides the danger of breaking the glaſs, when a large piece of that ſubſtance has acquired the degree of heat that is ſufficient for the purpoſe, it will retain that heat ſo long as to dry the ſealing-wax too much. With reſpect to the other method, when the ſealing-wax is diſſolved in very highly rectified ſpirits of wine, and the glaſs is previouſly warmed, it will anſwer tolerably well, but it is certainly inferior to the firſt method.

The electrical amber varniſh, when properly prepared, and when carefully laid upon the glaſs, will preſerve its inſulating property full as well as the ſealing-wax, which is applied by means of heat; but the preparation of this varniſh requires a great deal of attention; for if it be not properly made, the glaſs will not be in the leaſt improved

* Some perſons have uſed common ſpirit varniſh mixed with a little vermillion; but this ſort of covering ſerves more for ornament than for uſe, the glaſs being little if at all improved by it.

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by it; I ſhall therefore be as particular as I can in deſcribing the proceſs, and in pointing out the cauſes which are moſt likely to prevent its ſucceſs.

*Preparation of the Amber.*

Reduce ſome pieces of amber (the yellow amber is the beſt) into tolerably fine powder, and then melt it in an unglazed earthen veſſel over a charcoal fire;* when melted, pour it whilſt fluid upon an iron plate, and as ſoon as it is become cold, it muſt be pounded and ſifted through a very fine ſieve.

*Proceſs for making the Varniſh.*

Half a pint of linſeed oil, one ounce of *ſaccarum ſaturni* (ſugar of lead) and one ounce and a half of litharge, muſt be ſet to boil in an iron pot over a charcoal fire. As ſoon as the oil has diſſolved the two

* It muſt not be expected that the amber will melt into as fluid a ſtate as water or oil; for it will hardly acquire the fluidity of cold honey, and therefore it muſt not be kept upon the fire longer than may be neceſſary to produce that effect, otherwiſe it will be burned too much.

other

other ingredients, one ounce and a half of the prepared amber muſt be added, and then the whole muſt be left to boil to a proper degree of conſiſtence. Laſtly, it muſt be mixed with ſuch a quantity of ſpirit of turpentine as will bring it to the conſiſtence of olive oil.—I ſhall now ſubjoin the neceſſary precautions.

The capacity of the boiler ſhould be at leaſt four or five times greater than the bulk of materials, in order to allow for the ſwelling of the compoſition, which is very conſiderable towards the latter end. For the ſame reaſon the boiler muſt be conſtantly watched, and muſt be removed from the fire whenever the liquor comes near the top; for if any of it happen to run over, it will take fire immediately. The mixture ſhould be ſtirred every two or three minutes, but towards the latter end, viz. after it has boiled for about two hours, it ſhould be ſtirred oftener. An iron ladle is very fit for this purpoſe.

The conſiſtency of the liquor ſhews when it has boiled ſufficiently; for if at that

that time a drop of it be rubbed between two knife-blades, and the blades are afterwards ſeparated, the varniſh will ſtretch itſelf like a continued thread from one blade to the other. When this effect takes place, the pot may be removed from the fire, and left to cool, but before it becomes quite cold, the ſpirit of turpentine muſt be mixed with it. The beſt way of forming this mixture is to put the ſpirit of turpentine into a baſon or pipkin, then to add one ladle full of the varniſh at a time, and to ſtir it until it is quite diſſolved in the ſpirit. The varniſh may then be put in bottles, and kept for uſe.

If the compoſition has been boiled too much, the colour of the varniſh, when mixed with the ſpirit of turpentine, inſtead of a browniſh yellow, will be dark brown; and if it has not been boiled ſufficiently, the varniſh, when laid upon the glaſs, will always retain a degree of clammineſs. This clammineſs is likewiſe produced when the litharge and the ſaccarum ſaturni are not very dry; it will therefore be proper to dry thoſe articles

thoroughly

thoroughly previous to their being mixed with the oil.

This varniſh is laid upon the glaſs by means of a hair pencil, and one coat of it is quite ſufficient to preſerve the inſulating property of glaſs; but care ſhould be had to render the glaſs perfectly clean and dry, and to warm it previouſly to the application of the varniſh.

If the varniſh has been rightly prepared it will dry very ſpeedily; but for greater ſecurity it will be adviſeable to leave the varniſhed glaſs in a dry place for a day or two in the ſummer ſeaſon, and a little longer in the winter.

As ſome ingenious perſons may be in want of the beſt inſulating ſubſtances for nice electrical experiments, I think it not uſeleſs to mention, that raw ſilk poſſeſſes that property in a very eminent degree, even in damp and rainy weather. This ſubſtance, therefore, ſhould be preferred to all others, when ſtrength and pliableneſs

is

is required; otherwise sealing-wax and amber answer equally well.

Silks that have undergone the process of dying, or even of bleaching, though they insulate sufficiently well for common experiments, are, however, inferior to raw silk.

Quicksilver excites glass negatively.

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## N° X.

### *Addition to Page 35 of this Volume.*

" I find that Mr. FONTANA observed the action of the metallic application upon the heart a considerable time before Dr. FOWLER. In a letter to Mr. MARSIGLI, dated in November, 1792, which was printed at Pavia in a periodical publication, Mr. FONTANA, expresses himself thus, " Relativement au mouvement du
" cœur, je puis assurer, qu'il est facile d'ac-
" celerer ses battemens s'il est en mouve-
" ment, & de le remettre en mouvement
" s'il est en repos. Il suffit de le placer
" entre deux métaux, par example, le zinc

" & l'antimoine, de manière à ce qu'une
" partie de ce muſcle touche à l'un des
" metaux, & l'autre partie à l'autre metal.
" En faiſſant alors communiquer les deux
" metaux au moyen d'un conducteur, on
" verra s'effecteur les phénomènes que je
" viens d'indiquer, meme lorſque le cœur
" eſt ſeparé du corps & coupé par mor-
" ceaux. Je puis encore aſſurer que je fais
" contracter à volonté les vers de terre,
" les inſectes, & les animaux privés de cer-
" veau & de nerfs. Sous peu de tems je
" publierai un ouvrage ſur le nouveau
" principe du mouvement muſculaire, de-
" couvert à Bologne par le ſavant profeſſeur
" GALVANI, & j'eſpère demontrer d'une
" maniere rigoureuſe, que ce principe n'a
" rien de commun avec l'electricité, & que
" quel qu'il ſoit, il n'opère jamais la con-
" traction, ni ne reproduit jamais les mouve-
" mens muſculaires ordinaires aux animaux.
" Ainſi ce principe obſcur eſt réduit à un
" phénomène très-beau, mais dont la na-
" ture & les uſages reſtent encore à de-
" terminer *.

* See the *Journal de Phyſique* for March 1793.

N° XI.

## N° XI.

*Addition to Page* 61 *of this Volume.*

" The experiments of animal electricity are said to succeed also in the vacuum made by an air pump, viz. when the prepared animal and conducting rod are placed under the exhausted receiver, &c. — See the *Journal de Physique* for the year 1793, p. 290.

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